NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

Royal Institution of Great Britain,

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS.

VOL. II.

1854——1858.

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CONTENTS.

1854.

1855.

1856.

1857.

1858.

Royal Institution of Great Britain.

1854.

GENERAL MONTHLY MEETING,

Monday, November 6.

PROFESSOR C. WHEATSTONE, F.R.S. Vice-President, in the Chair.

The following Report was read:—

"*Royal Institution, November* 6, 1854.

"The MANAGERS Report,—That the Fullerian Professorship of Physiology is now vacant; and that, pursuant to the Deed of Endowment, the election of a Professor will take place on Monday, the 2nd of July, 1855, at 4 o'clock, P.M.

"They further Report,—That the next Actonian Prize of £105 will be awarded in the year 1858, to an Essay illustrative of the Wisdom and Beneficence of the Almighty as manifested by the Influence of Solar Radiation.—Competitors for this prize are requested to send their Essays to the Royal Institution, on or before 10 o'clock, P.M., December 31st, 1857, addressed to the Secretary. The adjudication will be made on Monday, April 12th, 1858."

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM

Her Majesty's Government (by Sir H. De la Beche)—Memoirs of the Geological Survey of the United Kingdom: British Organic Remains, Decades 4, 6, and 7. 4to. 1852-3.

Records of the School of Mines, Vol. I. Parts 2, 3, 4. 8vo. 1853.

Actuaries, Institute of—The Assurance Magazine, Nos. 16, 17. 8vo. 1854.

Agricultural Society of England, Royal—Journal, Vol. XV. Part 1. 8vo. 1854.

Airy, G. B. Esq. Astronomer-Royal—Description of the Transit Circle at the Royal Observatory, Greenwich. 4to. 1854.

American Academy of Arts and Sciences—Proceedings, Vol. II. Nos. 21-29. Vol. III. Nos. 1-13. 8vo. 1849-54.

American Philosophical Society—Proceedings, Nos. 49, 50. 8vo. 1853-4.

Amsterdam, Koninklijke Akademie van Wetenschappen—Verhandelingen, Eerste Deel. 4to. 1854.

Verslagen en Mededeelungen, Eerste Deel; en Tweede Deel, Eerste en Tweede Stuk. 8vo. 1853-4.

Arnold, Thos. J. Esq. (the Author)—Treatise on the Law relating to Municipal Corporations. 12mo. 1851.

Asiatic Society of Bengal—Journal, Nos. 240, 241. 8vo. 1854.
Asiatic Society, Royal—Journal, Vol. XVI. Part 1. 8vo. 1854.
Catalogue of Arabic and Persian MSS. in the Society's Library. By W. H. Morley. 8vo. 1854.
Astronomical Society, Royal—Memoirs, Vol. XXII. 4to. 1854.
Monthly Notices, Vol. XIII. Vol. XIV. Nos. 8, 9. 8vo. 1853-4.
Author—A Catechism explanatory of many of the commonly supposed Difficulties of Christianity. 12mo. 1854.
Basel, Naturforschende Gesellschaft—Berichte über die Verhandlungen, I-VIII. 8vo. 1835-49.
Verhandlungen, Heft I. 8vo. 1854.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal, August to November, 1854. 8vo.
Biber, Rev. G. E. LL.D. M.R.I. (the Author)—Literature, Art, and Science, considered as a means of elevating the Popular Mind. (A Lecture.) 8vo. 1854.
Bombay Geographical Society—Transactions, Vol. XI. 8vo. 1854.
Boosey, Messrs. (the Publishers)—The Musical World for July to Oct. 1854. 4to.
Boston Society of Natural History—Journal, Vol. VI. No. 3. 8vo. 1853.
Proceedings, Nos. 15-24. 8vo. 1852-4.
Botfield, B. Esq. F.R.S. M.R.I. (the Author)—Remarks on the Prefaces to the First Editions of the Classics. 8vo. 1854.
British Architects, Royal Institute of—Proceedings in July, 1854. 4to.
British Association—Report of the Twenty-third Meeting, held at Hull in 1853. 8vo. 1854.
Brown, Andrew, Esq. (the Author)—The Philosophy of Physics, or Process of Creative Developement. 8vo. New York, 1854.
Chemical Society—Quarterly Journal, Vol. VII. Nos. 2, 3. 8vo. 1854.
Commissioners in Lunacy—Eighth Report to the Lord Chancellor. 8vo. 1854.
Cornwall Polytechnic Society, Royal—Twenty-first Annual Report. 8vo. 1853.
Decimal Association—Proceedings, with an Introduction by Professor De Morgan. 8vo. 1854.
East India Company, the Hon.—Gazetteer of the Territories under the Government of the East India Company, and of the Native States on the Continent of India. By E. Thornton. 4 vols. 8vo. 1854.
Editors—The Medical Circular, for July to October, 1854. 8vo.
The Athenæum, for July to October, 1854. 4to.
The Practical Mechanic's Journal, for July to October, 1854. 4to.
The Mechanics' Magazine, for July to October, 1854. 8vo.
The Journal of Gas-Lighting, for July to October, 1854. 4to.
Deutsches Athenäum, July to October, 1854. 4to.
Ethnological Society of London—Address by Sir B. C. Brodie, Bart followed by a Sketch of the recent Progress of Ethnology, by R. Cull. 8vo. 1854.
Faraday, Professor, D.C.L. F.R.S.—Monatsbericht der Königl. Preuss. Akademie, Mai zu August, 1854. 8vo. Berlin.
Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin, 1853. 4to. 1854.
Kaiserliche Akademie der Wissenschaften, Wien:—
Philosophisch-Historische Classe:—Sitzungsberichte, Band XI. Hefte 4, 5; Band XII. Hefte 1-4; und Register zu Band I-X. 8vo. 1854.
Denkschriften. Band V. 4to. 1854.
Archiv für Kunde Œsterreichischer Geschichts Quellen. Band XII. 8vo. 1854.
Notizenblatt. (Beilage zum Archiv.) 1853. Nos. 21-24; 1854. Nos. 1-17.
Fontes Rerum Austriacarum. Zweite Abtheilung. Band I. 1854.
Mathematisch-Naturwissenschaftliche Classe:—Denkschriften. Band VII. 4to. 1854.
Sitzungsberichte. Band XI. Heft 5; Band XII. Hefte 1-4. 8vo. 1853-4.
Tafeln der Polygraphische Apparat. Von A. Auer. 8vo. 1853.

Faraday, Professor, D.C.L. F.R.S.—Bulletin de la Classe Physico-Mathématique de l'Académie Impériale des Sciences de St. Pétersbourg. Tome XII. 4to. 1854.
L'Académie Royale de la Belgique:—Annuaire. 1854. 8vo.
Bulletins des Séances de la Classe des Sciences, Année 1853. 8vo.
Mémoires présentés par divers Savans à l'Académie des Sciences de l'Institut Impérial de France: Sciences Mathématiques et Physiques. Tome XII. 4to. Paris, 1854.
Franklin Institute of Pennsylvania—Journal, Vol. XXVII. No. 6. Vol. XXVIII. No. 1-3. 8vo. 1854.
Genève, Société de Physique de—Mémoires, Tome XIII. 2me Partie. 4to. 1854.
Geographical Society, Royal—Address by the Earl of Ellesmere, May 1854. 8vo.
Geological Society—Quarterly Journal, No. 39. 8vo. 1854.
Graham, George, Esq. Registrar-General—Census of Great Britain, 1851: Population Tables: Ages, Condition, &c., Report and Summary Tables. fol. 1854.
Weekly Reports of the Registrar-General for July to October, 1854. 8vo.
Graham, Lt.-Colonel J. D. U.S. (the Author)—Report on the Boundary Line between the United States and Mexico. (Maps.) 8vo. 1853.
Greenwich Royal Observatory—Astronomical and Magnetical and Meteorological Observations in 1852. 4to. 1854.
Guggenbühl, Dr.—Die Cretinen: Heilanstalt. auf dem Abendberg in der Schweiz. Von Dr. Guggenbühl. 4to. Bern, 1853.
Una Visita all' Abendberg, 10 Settembre, 1850, del Medico Sella Alessandro. 8vo.
Cretins and Idiots—A Short Account of the Progress of the Institutions for their relief and cure. 8vo. 1853.
Horticultural Society of London—Journal, Vol. IX. No. 3. 8vo. 1854.
Huxley, Thomas H. F.R.S. (the Author)—On the Educational Value of the Natural History Sciences. 8vo. 1854.
Literary Fund, Royal—Report for 1854. 8vo.
Londesborough, Lord, K.C.H. M.R.I.—Miscellanea Graphica: a collection of Ancient, Mediæval, and Renaissance Remains in the possession of Lord Londesborough. Part 2. 4to. 1854.
Lovell, E. B. Esq. M.R.I. (the Editor)—The Monthly Digest, for July to October, 1854. 8vo.
The Common Law and Equity Reports. Parts 15-21. 8vo. 1854.
Lubbock, John, Esq. M.R.I.—On some Arctic Species of Calanidæ. 8vo. 1854.
Lyell, Sir Charles, F.R.S. (the Author)—Special Report on the Geological, Topographical, and Hydrographical Departments of the New York Industrial Exhibition for 1854. fol. 1854.
Melloni, Macédoine, F.R.S. Hon. M.R.I. (the Author)—La Thermochrôse, ou la Coloration Calorifique. Première Partie. 8vo. Naples, 1850.
Mure, Col. William, M.P. F.S.A. (the Editor)—Selections from the Family Papers preserved at Caldwell: 1496-1853. 3 vols. 4to. 1854.
Murchison, Sir R. I. D.C.L. F.R.S. M.R.I. (the Author)—Siluria. The History of the Oldest known Rocks containing Organic Remains. 8vo. 1854.
North of England Institute of Mining Engineers—Transactions, Vol. I. 8vo. 1852-3.
Norton, Mr. C. B. (the Publisher)—Norton's Literary Register, 1854. 16mo. New York.
Novello, Mr. (the Publisher)—The Musical Times, for July to October, 1854. 4to.
Photographic Society—Journal, Nos. 20-23. 8vo. 1854.

Quetelet, M. Hon.M.R.I. (*the Author*)—Sur le Climat de la Belgique: De l'Hygrométrie. 4to. Bruxelles, 1854.
Sur l'Électricité des Nuages orageux. 8vo.
Phénomènes Périodiques. 16mo. 1852.
Reid, P. Sandeman, Esq.—Letter on the Ventilation of Collieries. By John Buddle. 8vo. 1814.
Royal Society, London—Philosophical Transactions, Vol. CXLIV. Part 1. 4to. 1854.
Proceedings, Vol. VII. No. 4-6. 8vo. 1854.
Sabine, Col. E. R.A. V.P.R.S. (*the Author*)—On some of the Results obtained at the British Colonial Magnetic Observatories. 8vo. 1854.
Smith, Mr. J. Russell (*the Publisher*)—The Retrospective Review. Nos. 7, 8. 1854.
Smithsonian Institution, Washington—Smithsonian Contributions to Knowledge. Vol. VI. 4to. 1854.
Seventh Annual Report. 8vo. 1853.
Annular Eclipse of May 26, 1854. 8vo. 1854.
Società delle Scienze Biologiche in Torino—Memorie, Vol. I. Fascicolo 1. 8vo. 1854.
Society of Arts—Journal for July to October, 1854. 8vo.
Statistical Society—Journal, Vol. XVII. Part 3. 8vo. 1854.
Stephens, Henry, Esq. (*the Author*)—Cholera: an Analysis of its Character. 12mo. 1854.
Taylor, Rev. W. F.R.S. M.R.I.—Magazine for the Blind, August to November, 1854. 4to.
Von der Schlaflosigkeit, deren Ursachen und Heilart. Von Dr. A. F. Fischer. 16mo. Nürnberg, 1831.
Van Diemen's Land, Royal Society of—Papers and Proceedings. Vol. II. Part 2. 8vo. 1854.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Mai zu August, 1854. 4to. Berlin.
Warburton, Henry, Esq. M.A. F.R.S. M.R.I. (*the Author*)—On Self-repeating Series. 4to. 1854.
Washington National Observatory—Astronomical Observations made during 1847. 4to. 1853.
Webster, John, M.D. F.R.S. M.R.I.—General Reports of the Royal Hospitals of Bridewell and Bethlem, and of the House of Occupations. 8vo. 1853.
Williams, C. W. Esq. (*the Author*)—The Combustion of Coal and the Prevention of Smoke chemically and practically considered. 8vo. 1854.
Yates, James, Esq. M.A. F.R.S. M.R.I.—On the French System of Measures, Weights, and Coins; by James Yates, Esq. F.R.S. With an Abstract of the Discussion on the Paper, at the Society of Arts. 8vo. 1854.
Comparative Statement of Different Plans of Decimal Accounts and Coinage. By Th. W. Rathbone. 8vo. 1854.
Decimal Coinage—A Practical Analysis with Tables. By James Laurie. 8vo. 1854.
Yorkshire Geological and Polytechnic Society—Proceedings, 1853. 8vo. 1854.

GENERAL MONTHLY MEETING,

Monday, December 4.

William Robert Grove, Esq. Q.C. F.R.S. Vice-President, in the Chair.

The Earl of Rosse,
Benedict Laurence Chapman, Esq. and
Henry Pemberton, Esq.

were duly *elected* Members of the Royal Institution.

The Secretary reported that the following Arrangements had been made for the Lectures before Easter, 1855:—

Six Lectures on the Chemistry of Combustion (adapted to a Juvenile Auditory), by Michael Faraday, Esq. D.C.L. F.R.S. Fullerian Professor of Chemistry, R.I.

Eleven Lectures on Magnetism and Frictional Electricity, by John Tyndall, Esq. F.R.S. Professor of Natural Philosophy in the Royal Institution.

Eleven Lectures on English Literature, by William B. Donne, Esq.

Eleven Lectures on the Principles of Chemistry, by John Hall Gladstone, Ph. D. F.R.S.

The following Presents were announced, and the thanks of the Members returned for the same:—

From

Bell, Jacob, Esq. M.R.I.—The Pharmaceutical Journal, for November and December. 8vo. 1854.

Board of Trade—Report on the Bavarian Educational Institutions for Practical Science and Art. By J. Ward, Esq. fol. 1854.

Boosey, Messrs. (the Publishers)—The Musical World for November, 1854.

British Architects, Royal Institute of—Proceedings in November, 1854. 4to.

Civil Engineers, Institution of—Proceedings in November, 1854. 8vo.

East India Company, the Hon.—Catalogue of the Birds in the Museum of the Hon. East India Company. By Dr. Horsfield. Vol. I. 8vo. 1854.

Editors—The Medical Circular for November, 1854. 8vo.
The Athenæum for November, 1854. 4to.
The Practical Mechanic's Journal for November, 1854. 4to.
The Journal of Gas-Lighting, November, 1854. 4to.
The Mechanics' Magazine for November, 1854. 8vo.

Franklin Institute of Pennsylvania—Journal, Vol. XXVIII. No. 4. 1854.

Geological Society—Quarterly Journal, No. 40. 8vo. 1854.

Graham, George, Esq. (Registrar-General)—Weekly Reports of the Registrar-General for November, 1854. 8vo.

Jopling, R. F. Esq. (*the Editor*)—The Statist, No. 1. 8vo. 1854.
Medical and Chirurgical Society, Royal—Medico-Chirurgical Transactions, Vol. XXXVII. 8vo. 1854.
Novello, Mr. (*the Publisher*)—The Musical Times for November and December, 1854.
Photographic Society—Journal, No. 24. 8vo. 1854.
Pollock, Frederick, Esq. M.A. M.R.I.—Euleri Opuscula. 3 vols. 4to. Berolini, 1746–51.
Pollock, Thomas, Esq. (*the Author*)—On the peculiar State of the Atmosphere during the late Epidemic Cholera and Diarrhœa. 8vo. 1854.
Price, Rev. Bartholomew, M.A. F.R.S. (*the Author*)—A Treatise on the Infinitesimal Calculus. Vol. II. 8vo. 1854.
Rathbone, Th. W. Esq. (*the Author*)—Preface to the Comparative Statement on Decimal Accounts and Coinage. 8vo. 1854.
Society of Arts—Journal for November, 1854. 8vo.
Statistical Society—Journal, Vol. XVII. Part 4. 8vo. 1854.
Taylor, Alfred S. M.D. F.R.S. M.R.I. (*the Author*)—Medical Jurisprudence. 5th Edition. 16mo. 1854.
Taylor, Rev. W. F.R.S. M.R.I.—The Magazine for the Blind, December, 1854.
Wilhelm Von Humboldt—Lichtstrahlen aus seinen Briefen: mit einer Biographie Humboldts, von Elisa Maier. 16mo. Leipzig, 1852.
Ueber das Sehen und die Farben, eine Abhandlung von A. Schopenhauer. 8vo. Leipzig, 1816.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, September und October, 1854. 4to.

1855.

WEEKLY EVENING MEETING,

Friday, January 19.

William Robert Grove, Esq. Q.C. F.R.S. Vice-President, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On some points of Magnetic Philosophy.

The magnetic and electric forms of power, being dual in their character, and also able to act at a distance, will probably aid greatly in the development of the nature of physical force generally: and if (as I believe) the dualities are essential to the forces, are always equal and equivalent to each other, and are so mutually dependent, that one cannot appear, or even exist, without the other, the proof of the truth of such conditions would lead to many consequences of the highest importance to the philosophy of force generally. A few brief experiments with the electric power quickly place the dual

cases before the contemplative mind. Thus, if a metallic vessel, as an ice-pail, be insulated and connected with a delicate gold leaf electrometer, or other like instrument, and then an insulated metallic globe, half the diameter of the ice-pail, be charged with positive electricity and placed in the middle of the pail, the latter being for the moment uninsulated by a touch outside, and then left insulated again, the whole system will show no signs of electricity externally, nor will the electrometer be affected: but a carrier applied to the ball within the vessel will bring away from it positive electricity, showing its particular state of charge; or being applied to the lower inside surface of the vessel will bring away negative electricity, proving that it has the contrary state: or the duality may be proved by withdrawing the ball, when the vessel will show itself negative by the electrometer, and the ball will be found positive. That these dualities are equal, is further shown by replacing the ball within the vessel, observing the electrometer, bringing the ball and vessel in contact, and again observing the electrometer, which will remain unchanged; and finally withdrawing the ball, which comes away perfectly discharged, and leaves the vessel externally in its unchanged and previous state. So the electric dualities are equal, equivalent, and mutually sustained. To show that one cannot exist alone, insulate the metallic vessel, charge it strongly by contact with the machine or a Leyden jar, and then dip the insulated ball into it; and after touching the bottom of the vessel with the ball, remove it, without touching the sides: it will be found absolutely free from charge, whatever its previous state may have been: for none but a single state can exist at the bottom of such a metallic vessel; and a single state, *i.e.* an unrelated duality, cannot exist alone.

The correspondent dualities, *i.e.* the northness and the southness, of the magnetic force are well known. For the purpose of insulating, if possible, one of these, and separating it in any degree from the other, numerous experiments have been made. Thus six equal electro-magnets, formed of square bars, were put together in the direction of three lines perpendicular to each other, so that their inner ends, being all alike in polarity, might inclose a cubical space and produce an experimental chamber. When excited, these magnets were very powerful in the outer direction, as was found by nails, filings, spirals, and needles; but within the chamber, walled in on every side by intense north poles, there was no power of any kind: filings were not arranged; small needles not affected, except as they by their own inducing powers caused arrangement of the force within; revolving wire helices produced no currents: the chamber was a place of no magnetic action. Ordinary magnetic poles of like nature produced corresponding results. A single pole presented its usual character, attracting iron, repelling bismuth; a like pole, at right angles to it, formed a re-entering angle, and there a weak place of magnetic action was caused; iron was attracted from

it to the prominent corners; bismuth moved up into it; and a third like pole on the opposite side made the place of weak force still weaker and larger; another pole or two made it very weak; six poles brought it to the condition above described. Even four poles, put with their longer edges together, produced a lengthened chamber with two entrances; and a little needle being carried in at either entrance passed rapidly through spaces of weaker and weaker force, and found a part in the middle where magnetic action was not sensible.

Other very interesting results were obtained by making chambers in the polar extremities of electro-magnets. A cylinder magnet, whose core was 1·5 inches in diameter, had a concentric cylindrical chamber formed in the end, 0·7 in diameter, and 1·3 inches deep. When iron filings were brought near this excited pole, they clung around the outside, but none entered the cavity, except a very few near the outer edge. When they were purposely placed inside on a card they were quite indifferent to the excited pole, except that those near the mouth of the chamber moved out and were attracted to the outer edges. A piece of soft iron at the end of a copper wire was strongly attracted by the outer parts of the pole, but unaffected within. When the chamber was filled with iron filings and inverted, the magnet being excited, all those from the bottom and interior of the chamber fell out; many, however, being caught up by the outer parts of the pole. If pieces of iron, successively increasing from the size of a filing to a nail, a spike, and so on to a long bar, were brought into contact with the same point at the bottom of the inverted chamber, though the filing could not be held by attraction, nor the smaller pieces of iron, yet as soon as those were employed which reached to the level of the chamber mouth, or beyond it, attraction manifested itself; and with the larger pieces it rose so high that a bar of some pounds weight could be held against the very spot that was not sufficient to retain an iron filing.

These and many other results prove experimentally, that the magnetic dualities cannot appear alone; and that when they are developed they are in equal proportions and essentially connected. For if not essentially connected, how could a magnet exist alone? Its power, evident when other magnets, or iron, or bismuth is near it, must, upon their removal, then take up some *other form*, or exist *without action*: the first has never been shown or even suspected; the second is an impossibility, being inconsistent with the conservation of force. But if the dualities of a single magnet are thrown upon each other, and so become mutually related, is that in right lines through the magnet, or in curved lines through the space around? That it is not in right lines through the magnet (it being a straight bar or sphere) is shown by this, that the proper means as a helix round the magnet, shows that the *internal* disposition of the *force* (coercitive or other) is not affected when the magnet is exert-

ing its power on other magnets, or when left to itself (*Experimental Researches*, 3119, 3121, 3215, &c.); and like means show that the *external* disposition of the force is so affected: so that the force in right lines through the magnet does not change under the circumstances, whilst the force in external (and necessarily) curved lines does.

The polarity of bismuth or phosphorus in the magnetic field is one point amongst many others essentially dependent upon, and highly illustrative of the nature of, the magnetic force. The assumption that they have a polarity the reverse of that of paramagnetic bodies involves the consequence, that northness does not always repel northness or attract southness; or else leads to the assumption that there are two northnesses and two southnesses, and that these sometimes associate in pairs one way, and at other times in the contrary way. But leaving the assumptions and reverting to experiment, it was hoped that a forcible imitation of the imagined state of bismuth in the magnetic field, might illustrate its real state, and, for this purpose, recourse was had to the indications given by a moving conductor. Four spheres of copper, iron, bismuth, and hard steel have been prepared, and rotated upon an axis coincident with the magnetic axis of a powerful horse-shoe magnet; each sphere has a ring of copper fixed on it as an equator, and the ends of a galvanometer wire were brought into contact with the axis and the equator of the revolving globe. Under these circumstances, the electric current produced in the moving globe was conveyed to the galvanometer, and became the indicator of the magnetic polarity of the spheres; the direction of rotation, and the poles of the magnet, being in all cases the same. When the copper sphere, as a standard, was revolved, deflection at the galvanometer occurred in a certain direction. When the iron sphere replaced the copper and was revolved, the deflection at the galvanometer was the same. When the bismuth sphere was employed, the deflection was still the same:—and it still remained the same when the steel sphere was rotated in the magnetic field. Hence, by this effect, which I believe to be a truthful and unvarying indication of polarity, the state of all the spheres was the same, and therefore the polarity of the magnetic force in the iron, copper, and bismuth, in every case alike. (*Exp. Res.* 3164, &c.) The steel sphere was then magnetized in the direction of its axis, and was found to be so hard as to retain its own magnetic state when in a reverse direction between the poles of the dominant magnet, for upon its removal its magnetism remained unchanged. Experiments were then made in a selected position, where the dominant magnetic force was not too strong—(a magnet able to lift 430 lbs. was used) —and it was found that when the steel magnet was placed in accordance, *i.e.* with its north pole opposite the south pole of the dominant magnet, the deflection was in the same direction as with the bismuth sphere; but when it was changed so as to be in

the magnetic condition assigned by some to bismuth (*i.e.* with reversed polarities), it then differed from bismuth, producing the contrary deflection. [For a further account of these considerations and investigations, a paper may be referred to, which will appear in the February number of the Philosophical Magazine.]

It is, probably, of great importance that our thoughts should be stirred up at this time to a reconsideration of the general nature of physical force, and especially to those forms of it which are concerned in actions at a distance. These are, by the dual powers, connected very intimately with those which occur at insensible distances; and it is to be expected that the progress which physical science has made in latter times will enable us to approach this deep and difficult subject with far more advantage than any possessed by philosophers at former periods. At present we are accustomed to admit action at sensible distances, as of one magnet upon another, or of the sun upon the earth, as if such admission were itself a perfect answer to any enquiry into the nature of the physical means which cause distant bodies to affect each other; and the man who hesitates to admit the sufficiency of the answer, or of the assumption on which it rests, and asks for a more satisfactory account, runs some risk of appearing ridiculous or ignorant before the world of science. Yet Newton, who did more than any other man in demonstrating the law of action of distant bodies, including amongst such the sun and Saturn, which are 900 millions of miles apart, did not leave the subject without recording his well-considered judgment, that the mere attraction of distant portions of matter was not a sufficient or satisfactory thought for a philosopher. That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is, he says, to him a great absurdity. Gravity must be caused by an agent, acting constantly according to certain laws; but whether this agent be material or immaterial he leaves to the consideration of his readers. This is the onward looking thought of one, who by his knowledge and like quality of mind, saw in the diamond an unctuous substance coagulated, when as yet it was known but as a transparent stone, and foretold the presence of a combustible substance in water a century before water was decomposed or hydrogen discovered: and I cannot help believing that the time is near at hand, when his thought regarding gravity will produce fruit:—and, with that impression, I shall venture a few considerations upon what appears to me the insufficiency of the usually accepted notions of gravity, and of those forces generally, which are supposed to act at a distance, having respect to the modern and philosophic view of the conservation and indestructibility of force.

The notion of the gravitating force is, with those who admit

Newton's law, but go with him no further, that matter attracts matter with a strength which is inversely as the square of the distance. Consider, then, a mass of matter (or a particle), for which present purpose the sun will serve, and consider a globe like one of the planets, as our earth, either created or taken from distant space and placed near the sun as our earth is;—the attraction of gravity is then exerted, and we say that the sun attracts the earth, and, also, that the earth attracts the sun. But if the sun attracts the earth, that force of attraction must either arise *because* of the presence of the earth near the sun; or it must have *pre-existed* in the sun when the earth was not there. If we consider the first case, I think it will be exceedingly difficult to conceive that the sudden presence of an earth, 95 millions of miles from the sun, and having no previous physical connexion with it, nor any physical connexion caused by the mere circumstance of juxtaposition, should be able to raise up in the sun a power having no previous existence. As respects gravity, the earth must be considered as inert, previously, as the sun; and can have no more inducing or affecting power over the sun than the sun over it: both are assumed to be *without* power in the beginning of the case;—how then can that power arise by their mere approximation or co-existence? That a body without force should raise up force in a body at a distance from it, is too hard to imagine; but it is harder still, if that can be possible, to accept the idea when we consider that it includes the *creation of force*. Force may be opposed by force, may be diverted, directed partially or exclusively, may even be converted, as far as we understand the matter, disappearing in one form to reappear in another; but it cannot be created or annihilated, or truly suspended, *i.e.* rendered existent without action or without its equivalent action. The conservation of power is now a thought deeply impressed upon the minds of philosophic men; and I think that, as a body, they admit that the creation or annihilation of force is equally impossible with the creation or annihilation of matter. But if we conceive the sun existing alone in space, exerting no force of gravitation exterior to it; and then conceive another sphere in space having like conditions, and that the two are brought towards each other; if we assume, that by their mutual presence each causes the other to act,—this is to assume not merely a creation of power but a *double creation*, for both are supposed to rise from a previously inert to a powerful state. On their dissociation they, by the assumption, pass into the powerless state again, and this would be equivalent to the *annihilation* of force. It will be easily understood, that the case of the sun or the earth, or of any one of two or more acting bodies, is reciprocal;—and also that the variation of attraction, with any degree of approach or separation of the bodies, involves the same result of creation or annihilation of power as the creation or annihilation (which latter is only the total removal) of either of the acting bodies would do.

Such, I think, must be the character of the conclusion, if it be supposed that the attraction of the sun upon the earth arises *because* of the presence of the earth, and the attraction of the earth upon the sun, because of the presence of the sun: there remains the case of the power, or the efficient source of the power, having pre-existed in the sun (or the earth) *before* the earth (or the sun) was in presence. In the latter view it appears to me that, consistently with the conservation of force, one of three sub-cases must occur: either the gravitating force of the sun, when directed upon the earth, must be removed in an equivalent degree from some other bodies, and when taken off from the earth (by the disappearance of the latter) be disposed of on some other bodies;—or else it must take up some *new* form of power when it ceases to be gravitation, and consume some other form of power when it is developed as gravitation;—or else it must be *always* existing around the sun through infinite space. The first sub-case is not imagined by the usual hypothesis of gravitation, and will hardly be supposed probable: for, if it were true, it is scarcely possible that the effects should not have been observed by astronomers, when considering the motions of the planets in different positions with respect to each other and the sun. Moreover, gravitation is not assumed to be a dual power, and in them only as yet have such removals been observed by experiment or conceived by the mind. The second sub-case, or that of a new or another form of power, is also one which has never been imagined by others, in association with the theory of gravity. I made some endeavours, experimentally, to connect gravity with electricity, having this very object in view (*Phil. Trans.* 1851, p. 1); but the results were entirely negative. The view, if held for a moment, would imply that not merely the sun, but all matter, whatever its state, would have extra powers set up in it, if removed in any degree from gravitation; that the particles of a comet at its perihelion would have changed in character, by the conversion of some portion of their molecular force into the increased amount of gravitating force which they would then exert; and that at its aphelion, this extra gravitating force would have been converted back into some other kind of molecular force, having either the former or a new character: the conversion either way being to a perfectly equivalent degree. One could not even conceive of the diffusion of a cloud of dust, or its concentration into a stone, without supposing something of the same kind to occur; and I suppose that nobody will accept the idea as possible. The third sub-case remains, namely, that the power is always existing around the sun and through infinite space, whether secondary bodies be there to be acted upon by gravitation or not: and not only around the sun, but around every particle of matter which has existence. This case of a constant necessary condition to action in space, when as respects the sun the earth is *not* in place, and of a certain gravitating action as the result of that pre-

vious condition when the earth *is* in place, I can conceive, consistently, as I think, with the conservation of force: and I think the case is that which Newton looked at in gravity; is, in philosophical respects, the same as that admitted by all in regard to light, heat, and radiant phenomena; and (in a sense even more general and extensive) is that now driven upon our attention in an especially forcible and instructive manner, by the phenomena of electricity and magnetism, because of their dependence on dual forms of power.

Jan. 22, 1855. [M. F.]

WEEKLY EVENING MEETING,

Friday, January 26.

William Robert Grove, Esq. M.A. Q.C. F.R.S.
Vice-President, in the Chair.

Professor Tyndall, F.R.S.

On the Nature of the Force by which Bodies are Repelled from the Poles of a Magnet.

The Lecturer commenced, by showing that bodies are repelled by the poles of a magnet, in virtue of a state of excitement into which they are thrown by the latter. The repulsion of bismuth, and the attraction of soft iron, followed precisely the same laws when the strength of the influencing magnet was augmented, the respective forces being proportional, not simply to the strength, but within wide limits, to the square of the strength of the magnet. The result is explained in the case of iron by the fact of its being converted, while under magnetic influence, into a true temporary magnet, whose power varies with that of the influencing one; and in the case of bismuth, the result can only be explained by the fact that the dia-magnetic mass is converted into a true *dia-magnet.*

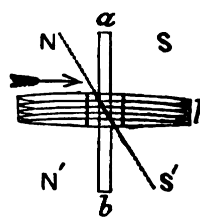
It was next shown that the condition of excitement evoked by a magnetic pole was not the same as that evoked by another pole of an opposite quality. If the repulsion were independent of the quality of the pole, then two poles of unlike names ought to repel the bismuth, when brought to act upon it simultaneously. This is not the case. Two poles of the same name produce repulsion; but when they are of equal powers and opposite names, the condition excited by one of them is neutralized by the other, and no repulsion follows.

Bars of magnetic and dia-magnetic bodies were next submitted

to all the forces capable of acting upon them magnetically; first, to the magnet alone; secondly, to the electric current alone; and, thirdly, to the magnet and current combined. Attention to structure was here found very necessary, and the neglect of it appears to have introduced much error into this portion of science. Powdered bismuth, without the admixture of any foreign ingredient, was placed in a strong metallic mould, and submitted to the action of a hydraulic press; perfectly compact metallic masses were thus procured, which, suspended in the magnetic field with the line of compression horizontal, behaved exactly like magnetic bodies, setting their longest dimensions from pole to pole. This identity of deportment with an ordinary magnetic substance was also exhibited in the case of the electric current, and of the current and the magnet combined. In like manner, by the compression of a magnetic powder magnetic bars were produced, which, between the two poles of a magnet, set exactly like ordinary dia-magnetic ones; this identity of deportment is preserved when the bars are submitted to the action of the current, and of the current and magnet combined. Calling those bars which show the ordinary magnetic and dia-magnetic action *normal bars*, and calling the compressed bars *abnormal ones*, the law follows, that an abnormal bar of one class of bodies exhibits precisely the same deportment, in all cases, as the normal bar of the other class; but when we compare normal bars of both classes together, or abnormal bars of both classes, then the antithesis of action is perfect. The experiments prove that, if that which Gauss calls the *ideal distribution* of magnetism in magnetic bars be inverted, we have a distribution which will produce all the phenomena of dia-magnetic ones.

The important question of dia-magnetic polarity was submitted to further and stricter examination. A flat helix, whose length was an inch, internal diameter an inch, and external diameter seven inches, was attached firmly to a table, with its coils vertical. A suspension was arranged by means of which a bar of bismuth, five inches long, and 0·4 of an inch in diameter, was permitted to swing freely, while surrounded by the helix. With this arrangement, the following experiments were, or might be made:—1. A voltaic current from twenty of Grove's cells was sent through the helix *h*, the direction of the current *in the upper half* of the helix being that denoted by the arrow (Fig. 1). The north pole of a magnet being placed at N, the end *a* of the suspended bar of bismuth, *a b*, was *attracted* towards the pole N. 2. The south pole of a second magnet being placed at S, and the current being sent through the helix in the same direction as before, the bar left its central position and approached N with greater force than in the

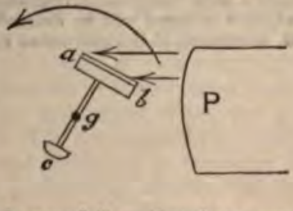
Fig 1.



former experiment. The reason was deemed manifest: the state of excitement which causes *a* to be attracted by N causes it to be repelled by S; both poles, therefore, act in unison, and a deflection of greater energy is produced. 3. The pole S being removed to the position S′, the deflection was also found to be about twice as forcible as when the single pole N was employed. Here also the reason is plain: the two ends, *a* and *b*, of the bismuth bar, are in different states of excitement; the end *a* is attracted by a north pole, the end *b* is attracted by a south pole: both poles act therefore as a mechanical couple upon the bar, and produce the deflection observed. 4. The pole S′ was replaced by a north pole of the same strength, thus bringing two poles of the same name to bear upon the two ends of the bar: there was no deflection by this arrangement; it is manifest that N's attraction for the end *a* was nullified by the repulsion of the end *b* by a like pole; the experiment thus furnishes an additional proof of the polar condition of *a b*. 5. We have supposed the pole S to be removed into the position S′; but permitting the pole S to remain, and introducing another pole (a south one) at S′, a greater action than that produced with two magnets was obtained. 6. Finally, adding another north pole at N′, and allowing four magnets to operate upon the bismuth bar simultaneously, a maximum action was obtained, and the bar was attracted and repelled with the greatest promptness and decision. *In all these cases where an iron bar was substituted for the bismuth bar a b, a deflection precisely the opposite to that exhibited by a b was produced.* A branch of the current by which the bar of bismuth was surrounded could be suffered to circulate round a bar of iron, suspended freely in an adjacent helix; when the forces acting upon the iron were the same as those acting upon the bismuth, the bars were always deflected in opposite directions.

The question of dia-magnetic polarity was next submitted to a test which brought it under the dominion of the principles of mechanics. A mass of iron was chosen for the moveable magnetic pole, of such a shape that the diminution of the force emanating from the pole, as the distance was augmented, was very slow; or in other words, the field of force was very uniform. Let the space in front of the pole P, (Fig. 2.) be such a field. A *normal* bar of bismuth, *a b*, was attached to the end of a lever transverse to the length of the latter, and counterpoised by a weight at the other extremity: the system was then suspended from its centre of gravity *g*, so that the beam and bar swung horizontally. Supposing the bar to occupy the position shown in the figure, then if the force acting upon it *be purely repulsive*—that is to say, if the dia-magnetic force be unpolar—it is evident that the tendency

Fig. 2.



of the force acting upon every particle of the mass of bismuth tends to turn the lever round its axis of suspension, in the direction of the curved arrow. On exciting the magnetism of P, however, a precisely contrary motion is observed—*the lever approaches the pole.* This result, which, as far as the lecturer could see, was perfectly inexplicable on the assumption that the dia-magnetic force was purely repulsive, is explained in a simple and beautiful manner on the hypothesis of dia-magnetic polarity. According to this, the end *b* of the bar of bismuth is repelled by P, and the end *a* is attracted: but the force acting upon *a* is applied at a greater distance from the axis of suspension than that acting upon *b*; and as it has been arranged that the absolute intensities of the forces acting upon the two ends differ very slightly from each other, the mechanical advantage possessed by *a* gives to it the greatest moment of rotation, and the bar is attracted instead of repelled. Let a magnetic needle *n s* (Fig. 3,) be attached like the bar *a b* (Fig. 2) to a lever, and submitted to the earth's magnetism. Let the north pole of the earth be towards N; the action of the pole upon *n* is attractive, upon *s* repulsive, the absolute intensities of these forces are the same, inasmuch as the length of the needle is a vanishing quantity in comparison with its distance from the pole N: hence the mechanical advantage possessed by the force acting upon *s*, on account of its greater distance from the axis of rotation, causes the lever to recede from N, and we obtain a result perfectly analogous to that obtained with the bar of bismuth (Fig. 2).*

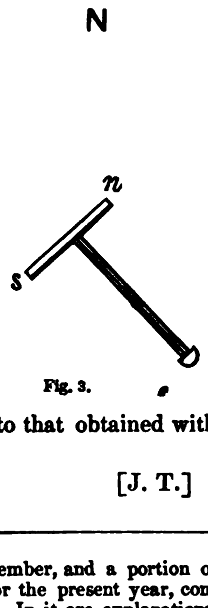


Fig. 3.

[J. T.]

* A paper submitted to the Royal Society last November, and a portion of which formed the subject of the Bakerian Lecture for the present year, contains a more comprehensive discussion of this subject. In it are explanations, which it is hoped will be deemed satisfactory, of the difficulties adduced by M. Matteucci, in his instructive *Cours Special*, recently published.

WEEKLY EVENING MEETING,

Friday, February 2.

Sir Henry Holland, Bart. M.D. F.R.S.
Vice-President, in the Chair.

G. B. Airy, Esq. F.R.S. Astronomer-Royal,

On the Pendulum-experiments lately made in the Harton Colliery, for ascertaining the mean Density of the Earth.

The speaker commenced with remarking that the bearing of the experiments, of which he was about to give a notice, was not limited to their ostensible object, but that it applied to all the bodies of the Solar system. The professed object of the experiments was to obtain a measure of the density of the earth, and therefore of the mass of the earth (its dimensions being known); but the ordinary data of astronomy, taken in conjunction with the laws of gravitation, give the proportions of the mass of the earth to the masses of the sun and the principal planets; and thus the determination of the absolute mass of the earth would at once give determinations of the absolute masses of the sun and planets. To show how this proportion is ascertained, it is only necessary to remark, that a planet, if no force acted on it, would move in a straight line; that, therefore, if we compute geometrically how far the planet moves in a short time, as an hour, and then compute the distance between the point which the planet has reached in its curved orbit, and the straight line which it has left, we have found the displacement which is produced by the sun's attraction, and which is therefore a measure of the sun's attraction. In like manner, if we apply a similar calculation to the motion of a satellite during one hour, we have a measure of the attraction of its primary. The comparison of these two gives the proportion of the attraction of the sun, as acting upon a body, at one known distance, to the attraction of a planet, as acting upon a body at another known distance. It is then necessary to apply one of the theorems of the laws of gravitation, namely, that the attraction of every attracting body is inversely as the square of the distance of the attracted body; and thus we obtain the proportion of the attractions of the sun and a planet, when the bodies upon which they are respectively acting are at the same distance from both: and finally, it is necessary to apply another theorem of the law of gravitation, namely, that the attractions thus found, corresponding to equal distances of the

attracted bodies, are in the same proportion as the masses of the attracting bodies (a theorem which applies to gravitation, but does not apply to magnetic and other forces). Into the evidence of these portions of the law of gravitation, the speaker did not attempt to enter: he remarked only that they rest upon very complicated chains of reasoning, but of the most certain kind. His only object was to show that the proportion of the masses of all bodies, which have planets or satellites revolving round them, can easily be found—(the proportion for those which have no satellites is found by a very indirect process, and with far less accuracy); and that if the absolute mass of the earth be known, the absolute mass of each of the others can be found. As their dimensions are known, their densities can then be found. Thus it rests upon such inquiries as those on which this discourse is to treat, to determine (for instance) whether the planet Jupiter is composed of materials as light as water, or as light as cork.

The obvious importance of these determinations had induced philosophers long since to attempt determinations of the earth's density: and two classes of experiments had been devised for it.

The first class (of which there was only one instance) is the attraction of a mountain, in the noble Schehallien experiment. It rests, in the first place, upon the use of the zenith sector; and, in the next place, upon our very approximate knowledge of the dimensions of the earth. [The construction of the zenith sector was illustrated by a model: and it was shown, that if the same star were observed at two places, the telescope would necessarily be pointed in the same direction at the two places, and the difference of direction of the plumb line, as shown by the different points of the graduated arc which it crossed at the two places, would show how much the direction of gravity at one place is inclined to the direction of gravity at the other place.] Now, from our knowledge of the form and dimensions of the earth, we know that the direction of gravity changes very nearly one second of angle for every 100 feet of horizontal distance. Suppose then, that two stations were taken on Schehallien, one on the north side and the other on the south side, and suppose that their distance was 4000 feet; then, if the direction of gravity had not been influenced by the mountain, the inclination of the directions of gravity at these two places would have been about 40 seconds. But suppose, on applying the zenith sector in the way just described, the inclination was found to be really 52 seconds. The difference, or 12 seconds, could only be explained by the attraction of the mountain, which, combined with what may be called the natural direction of gravity, produced directions inclined to these natural directions. In order to infer from this the density of the earth, a calculation was made (founded upon a very accurate measure of the mountain) of what would have been the disturbing effect of the mountain if the mountain had been as dense as the interior of the earth. It was found that the dis-

turbance would have been about 27 seconds. But the disturbance was really found to be only 12 seconds. Consequently the proportion of the density of the mountain to the earth's density was that of 12 to 27, or 4 to 9 nearly. And from this, and the ascertained density of the mountain, it followed that the mean specific gravity of the earth would be about five times that of water. The only objection to this admirable experiment is, that the form of the country near the mountain is very irregular, and it is difficult to say how much of the 12 seconds is or is not really due to Schehallien.

The second class is what may be called a cabinet experiment, possessing the advantage of being extremely manageable, and the disadvantage of being exceedingly delicate, and liable to derangement by forces so trifling that they could with difficulty be avoided. Two small balls upon a light horizontal rod were suspended by a wire, or two wires, forming a torsion balance, and two large leaden balls were brought near to attract the small balls from the quiescent position. We could make a calculation of how far the great balls would attract the little ones, if they were as dense as the general mass of the earth; and comparing this with the distance to which the leaden balls really do attract them, we find the proportion of the density of the earth to the density of lead. The peculiar difficulty and doubt of the results in this experiment depend on the liability to disturbances from other causes than the attraction of the leaden balls, especially the currents of air produced by the approach of bodies of a different temperature; and after all the cautions of Cavendish, Reich, and Baily, in their successive attempts, it seems not impossible that the phænomena observed may have been produced in part by the temperature of the great balls as well as their attraction.

These considerations induced Mr. Airy, in 1826, to contemplate a third class of experiments, namely, the determination of the difference of gravity at the top and the bottom of a deep mine, by pendulum experiments. Supposing the difference of gravity found, its application to the determination of density (in the simplest case) was thus explained. Conceive a spheroid concentric with the external spheroid of the earth to pass through the lower station in the mine. It is easily shown that the attraction of the shell included between these produces no effect whatever at the lower station, but produces the same effect at the upper station as if all its matter were collected at the earth's centre. Therefore, at the lower station we have the attraction of the interior mass only: at the upper station we have the attraction of the interior mass (though at a greater distance from the attracted pendulum) and also the attraction of the shell. It is plain that by making the proportion of these theoretical attractions equal to the proportion actually observed by means of the pendulum, we have the requisite elements for finding the proportion of the shell's attraction to the internal mass's attraction, and therefore the proportion of the matter in the shell to the

matter in the internal mass; from which the proportion of density is at once found. Moreover, it appeared probable, upon estimating the errors to which observations are liable, that the resulting error in the density, in this form of experiment, would be less than in the others.

Accordingly, in 1826, the speaker, with the assistance of his friend Mr. Whewell (now Dr. Whewell), undertook a series of experiments at the depth of nearly 1200 feet, in the Dolcoath mine, near Camborne, in Cornwall. The comparison of the upper and lower clocks (to which further allusion will be made) was soon found to be the most serious difficulty. The personal labour was also very great. They had, however, made a certain progress when, on raising a part of the instruments, the straw packing took fire—(the origin of the fire is still unknown),—and partly by burning, and partly by falling, the instruments were nearly destroyed.

In 1828 the same party, with the assistance of Mr. Sheepshanks and other friends, repeated the experiment in the same place. After mastering several difficulties, they were stopped by a slip of the solid rock of the mine, which deranged the pumps and finally flooded the lower station.

The matter rested for nearly twenty-six years, the principal progress in the subjects related to it being the correction to the computation of "buoyancy" of the pendulum, determined by Colonel Sabine's experiments. But in the spring of 1854, the manipulation of galvanic signals had become familiar to the Astronomer Royal, and the Assistants of the Greenwich Observatory, and it soon occurred to him that one of the most annoying difficulties in the former experiment might be considered as being practically overcome, inasmuch as the upper and lower clocks could be compared by simultaneous galvanic signals. Inquiries, made in the summer, induced him to fix on the Harton colliery near South Shields, where a reputed depth of 1260 feet could be obtained; and as soon as this selection was known, every possible facility and assistance were given by the owners of the mine. Arrangements were made for preparing an expedition on a scale sufficient to overcome all anticipated difficulties. A considerable part of the expense was met by a grant from the Board of Admiralty. The Electric Telegraph Company, with great liberality, contributed (unsolicited) the skill and labour required in the galvanic mountings. The principal instruments were lent by the Royal Society. Two observers were furnished by the Royal Observatory, one by the Durham Observatory, one by the Oxford Observatory, one by the Cambridge Observatory, and one by the private observatory of Red Hill (Mr. Carrington's). Mr. Dunkin, of the Royal Observatory, had the immediate superintendence of the observations.

The two stations selected were exactly in the same vertical, excellently walled, floored, and ceiled; the lower station, in particular, was a most comfortable room or rather suite of rooms. Every

care was taken for solidity of foundation and steadiness of temperature. In each (the upper and the lower) was mounted an invariable brass pendulum, vibrating by means of a steel knife edge upon plates of agate, carried by a very firm iron stand. Close behind it, upon an independent stand, was a clock, carrying upon the bob of its pendulum an illuminated disk, of diameter nearly equal to the breadth of the tail of the invariable pendulum; and between the two pendulums was a chink or opening of two plates of metal, which admitted of adjustment, and was opened very nearly to the same breadth as the disk. To view these, a telescope was fixed in a wall, and the observer was seated in another room. When the invariable pendulum and the clock pendulum pass the central points of vibration at the same instant, the invariable pendulum hides the illuminated disk as it passes the chink, and it is not seen at all. At other times it is seen in passing the chink. The observation, then, of this disappearance determines a coincidence with great precision. Suppose the next coincidence occurs after 400 seconds. Then the invariable pendulum (swinging more slowly), has lost exactly two swings upon the clock pendulum, or the proportion of its swings to those of the clock pendulum is 398 : 400. If an error of a second has been committed, the proportion is only altered to 397 : 399, which differs by an almost insignificant quantity. Thus the observation, in itself extremely rude, gives results of very great accuracy. As the proportion of invariable-pendulum-swings to clock-pendulum-swings is thus found, and as the clock-pendulum-swings in any required time are counted by the clock dial, the corresponding number of invariable-pendulum-swings is at once found. Corrections are then required for the expansion of the metal (depending on the thermometer-reading), for the arc of vibration, and for the buoyancy in air (depending on the barometer-reading).

But when the corrected proportion of upper-invariable-pendulum-swings to upper-clock-pendulum-swings is found, and the proportion of lower-invariable-pendulum-swings to lower clock-pendulum-swings is found, there is yet another thing required:—namely, the proportion of upper-clock-pendulum-swings to lower-clock-pendulum-swings in the same time; or, in other words, the proportion of the clock rates. It was for this that the galvanic signals were required. A galvanometer was attached to each clock, and an apparatus was provided in a small auxiliary clock, which completed a circuit at every 15 seconds nearly. The wire of this circuit, passing from a small battery through the auxiliary clock, then went through the upper galvanometer, then passed down the shaft of the mine to the lower galvanometer, and then returned to the battery. At each galvanometer there was a small apparatus for breaking circuit. At times previously arranged, the circuit was completed by this apparatus at both stations, and then it was the duty of the observers at both stations to note the clock times of

the same signals; and these evidently give comparisons of the clocks, and therefore give the means of comparing their rates. Thus (by steps previously explained), the number of swings made by the upper pendulum is compared with the number of swings made in the same time by the lower pendulum.

Still the result is not complete, because it may be influenced by the peculiarities of each pendulum. In order to overcome these, after pendulum A had been used above and pendulum B below, they were reversed; pendulum B being observed above and A below; and this, theoretically, completes the operation. But in order to insure that the pendulum received no injury in the interchange, it is desirable again to repeat the experiments with A above and B below, and again with B above and A below.

In this manner the pendulums were observed with 104 hours of incessant observations, simultaneous at both stations, A above and B below; then with 104 hours, B above and A below; then with 60 hours, A above and B below; then with 60 hours, B above and A below. And 2454 effective signals were observed at each station.

The result is, that the pendulums suffered no injury in their changes; and that the acceleration of the pendulum on being carried down 1260 feet is $2\frac{1}{4}$ seconds per day, or that gravity is increased by —$\frac{1}{19190}$ part.

It does not appear likely that this determination can be sensibly in error. The circumstances of experiment were, in all respects, extremely favourable; the only element of constant error seems to be that (in consequence of the advanced season of the year), the upper station was cooler by 7° than the lower station, and the temperature-reductions are therefore liable to any uncertainty which may remain on the correction for 7°. The reductions employed were those deduced by Sabine from direct experiment, and their uncertainty must be very small.

If a calculation of the earth's mean density were based upon the determination just given, using the simple theory to which allusion is made above, it would be found to be between six times and seven times the density of water. But it is necessary yet to take into account the deficiency of matter in the valley of the Tyne, in the hollow of Jarrow Slake, and on the sea-coast. It is also necessary to obtain more precise determinations of the specific gravities of the rocks about Harton colliery than have yet been procured. Measures are in progress for supplying all these deficiencies. It seems probable that the resulting number for the earth's density will probably be diminished by these more accurate estimations.

[G. B. A.]

GENERAL MONTHLY MEETING,

Monday, February 5.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Edmund Macrory, Esq.
Henry Maudslay, Esq.
Hensleigh Wedgwood, Esq., and
John William Wrey, Esq. M.A.

were duly *elected* Members of the Royal Institution.

Richard Hoper, Esq., and
Henry Pemberton, Esq.

were *admitted* Members of the Royal Institution.

The following Presents were announced, and the thanks of the Members returned for the same:—

From

Her Majesty's Government—Catalogues of the Stars near the Ecliptic observed at Markree, in 1852–4. 8vo. 1854.

Actuaries, Institute of—The Assurance Magazine, No. 18. 8vo. 1854.
Andrews, J. R. Esq. (the Author)—Four Months' Tour in the East. 12mo. 1853.
Art-Union of London—Report for 1854. 8vo. Almanacs for 1855. 32mo.
Asiatic Society of Bengal—Journal, Nos. 242, 243. 8vo. 1854.
Astronomical Society—Monthly Notices. Vol. XV. Nos. 1, 2. 8vo. 1854–5.
Bache, Dr. A. D. (the Author)—Annual Report of the Superintendent of the Coast Survey (of the United States), 1851 and 1852. With Charts. 1 vol. 8vo. and 2 vols. 4to. Washington, 1852-3
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal, Dec. 1854, and Jan. 1855. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for December, 1854, and January, 1855. 4to.
British Architects, Royal Institute of—Proceedings in December, 1854, and January, 1855. 4to.
Brodhurst, Bernard E. Esq. M.R.I. (the Author)—The Lateral Curvature of the Spine, its Pathology and Treatment. 16mo. 1855.
Brodie, Sir Benjamin C. Bt. D.C.L. F.R.S. M.R.I. (the Author)—Psychological Enquiries: in a Series of Essays intended to illustrate the Mutual Relations of the Physical Organisation and the Mental Faculties. 2nd Edition. 16mo. 1855.
Chemical Society—Quarterly Journal. No. 28. 8vo. 1854.
Civil Engineers, Institution of—Proceedings for Dec. 1854, and Jan. 1855. 8vo.
Cox, Alfred, Esq. (the Author)—The Landlord's and Tenant's Guide. 8vo. 1853.

East India Company, Hon.—Meteorological Observations at Madras in 1846–50. 4to. 1854.

Editors—The Medical Circular for December, 1854, and January, 1855. 8vo.
The Athenæum for December, 1854, and January, 1855. 4to.
The Practical Mechanic's Journal for December, 1854, and Jan. 1855. 4to.
The Mechanic's Magazine for December, 1854, and January, 1855. 8vo.
The Journal of Gas-Lighting for December, 1854, and January, 1855. 4to.
Deutsches Athenäum, for December, 1854, and January, 1855. 4to.
The Church of England Quarterly Review, January, 1855. 8vo.

Faraday, Professor, D.C.L. F.R.S.—Monatsbericht der Königl. Preuss. Akademie, September zu November, 1854. 8vo. Berlin.
Kaiserliche Akademie der Wissenschaften, Wien:—
Philosophisch-Historische Classe:—Sitzungsberichte. Band XII. Heft 5. 8vo. 1854.
Archiv für Kunde Œsterreichischer Geschichts-Quellen. Band XIII. 8vo. 1854.
Notizenblatt. (Beilage zum Archiv.) 1854. Nos. 18–24. 8vo.
Monumenta Habsburgica. Erste Abtheilung. Band I. 8vo. 1854.
Mathematisch-Naturwissenschaftliche Classe:—Sitzungsberichte. Band XII. Heft 5; Band XIII.; und Register zu Bande I–X. 8vo. 1854.
Geognostische Karte der Umgebungen von Krems und vom Manhardsberge. Von J. Czjek. 1854.
Memoria sobre las causas Meteorologico-Fisícas que producen las constantes sequías de Murcia y Almeria: su Autor Don Manuel Rico y Sinobas. 8vo. Madrid, 1851.
Etudes Chimiques sur les Eaux Pluviales et sur l'Atmosphère de Lyon, &c. Par A. Bineau. 8vo. Lyon, 1854.
Mémoires relatifs à l'Assainissement des Ateliers, des Edifices publics et des Habitations particulières. Par J. P. J. D'Arcet. 4to. Paris, 1853.
Rapport sur les Tapisseries et les Tapis des Manufactures Nationales, &c. Par M. Chevreul. 8vo. Paris, 1854.
An Essay explanatory of the Tempest Prognosticator. By G. Merryweather, M.D. (the Inventor). 8vo. 1851.
Maritime Conference held at Brussels for devising a uniform System of Meteorological Observations at Sea. 4to. 1853.

Franklin Institute of Pennsylvania—Journal, Vol. XXVIII. Nos. 5, 6. 8vo. 1854.

Graham, George, Esq. Registrar-General—Report of the Registrar-General for December, 1854, and January, 1855. 8vo.

Granville, A. B. M.D. F.R.S. M.R.I. (the Author)—Sudden Death. 12mo. 1855.

Hargraves, E. H. Esq. (the Author)—Australia and its Gold Fields. 16mo. 1855.

Highley, Mr. (the Publisher)—Botanical Letters to a Friend, by Dr. F. Unger, translated by D. B. Paul. 16mo. 1853.
The Microscope, in its adaptation to Vegetable Anatomy and Physiology: by Dr. H. Schacht; translated by F. Currey. 16mo. 1853.

Leake, Lieut.-Colonel W. M. F.R.S. M.R.I. (the Author)—A Letter to Col. Chesney, and a Postscript. 8vo. 1854-5.

Linnean Society of London—Transactions, Vol, XXI. Part 3. 4to. 1854.
Proceedings, Nos. 52-58. 8vo. 1853-4.
Annual Address of the President, 1854. 8vo.

Liverpool Literary and Philosophical Society—Proceedings, No. 8. 8vo. 1854.

London University—London University Calendar, 1855. 12mo.

Lovell, E. B. Esq. M.R.I. (the Editor)—The Common Law and Equity Reports. Part XXII. 8vo. 1854.

Marcet, W. M.D. (the Author)—Account of the Organic Chemical Constituents, or Immediate Principles of the Excrements of Man and Animals in the Healthy State. (From Phil. Trans. Roy. Soc. 1854.) 4to. 1854.

Morley, R. Reginald, Esq. M.R.I.—Descriptive Catalogue of Historical MSS. in the Arabic and Persian Languages in the Library of the Royal Asiatic Society. By W. H. Morley. 8vo. 1854.
Digest of Indian Cases. New Series Vol. I. By W. H. Morley. 8vo. 1852.
Morris, H. Sutherland, Esq., M.R.I.—Vesalii Opera Omnia Anatomica et Chirurgica curâ H. Boerhaave et B. S. Albini. 2 vols. fol. Lug. Bat. 1725.
Novello, Mr. (the Publisher)—The Musical Times for December, 1854, and January, 1855. 4to.
Orr, Messrs. and Co. (the Publishers)—The Theory and Practice of Landscape Painting in Water Colours. By G. Barnard. 8vo. 1855.
Petermann, A. Esq. (the Author)—Karte des Europäischen Russlands und der Angrenzenden Länder. 1855.
Photographic Society—Journal, Nos. 24-26. 8vo. 1854-5.
Quaritch, Mr. B. (the Publisher)—Early Christianity in Arabia: an Historical Essay. By T. Wright, Esq. F.S.A. 8vo. 1855.
Reeves, Evans, M.D. (the Author)—Diseases of the Stomach. 12mo. 1854.
Reid, P. Sandeman, Esq. (the Author)—On Practice with Gas at Blowers. 8vo. 1854.
Royal Society of Edinburgh—Transactions, Vol. XXI. Part 1. 4to. 1854.
Proceedings, No. 44. 8vo. 1853-4.
Scoffern, John, M.B. (the Editor)—The Subject-Matter of a Course of Ten Lectures on Organic Chemistry applied to the Arts, delivered at the Royal Institution in 1852, by W. T. Brande, Esq. 16mo. 1854.
Society of Arts—Journal for December, 1854, and January, 1855. 8vo.
Soret, M. L. (the Author)—Sur la Décomposition des Sels du Cuivre par la Pile. 8vo. 1854.
Sylvester, J. J. M.A. F.R.S. (the Author)—A Probationary Lecture on Geometry, delivered before the Gresham Committee, Dec. 4th. 8vo. 1854.
Taylor, Rev. W. F.R.S. M.R.I.—Magazine for the Blind, December, 1854, and January, 1855. 4to.
Thoughts and Fancies in Solitary Hours. By C. R. Lambert. 8vo. 1840.
Zopf und Schwert. Historisches Lustspiel von K. Gutzkow. 16mo. Leipzig, 1850.
Vincent, Mr. B. (Assistant Secretary, R.I.)—Döderlein's Handbook of Latin Synonyms. Translated by the Rev. H. H. Arnold. 8vo. 1841.
Essay on Education, by J. Whitchurch. 16mo. 1772.
Voyage Pittoresque des Environs de Paris. Par M. D***. 12mo. Paris, 1762.
Traité Elémentaire de l'Imprimerie. Par A. F. Momoro. 8vo. Paris, 1786.
Wrey, John W. Esq. M.A. M.R.I.—The Morality of Public Men. A Letter to the Earl of Derby. 8vo. 1852.
"Sans Changer," the real Basis of "The Morality of Public Men." A Snowball for "an Englishman." Thrown by I. W. W. 8vo. 1853.

WEEKLY EVENING MEETING,

Friday, February 9.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Owen, F.R.S.

On the Anthropoid Apes, and their relations to Man.

In this discourse, the structure, more especially of the bones and teeth, of the most highly organised Apes—the Orang-utans and Chimpanzees—was compared with that of the Human Subject, in reference to the hypothesis that specific characters can be so far modified by external influences, operating on successive generations, as to produce a new and higher species of animal, and that thus there had been a gradual progression from the monad up to man.

The conditions under which an active monad might be developed from dead mucus, or other organic matter in an infusion, or those that might influence and attend the transmutation of one recognised species into another—say of a polype into a medusa—were legitimate and important subjects of physiological research. But, hitherto, the results of such researches had not favoured the hypothesis of the coming in of species by spontaneous generation and transmutation.

The last link in the chain of changes—from *Quadrumana* to *Bimana*—according to the latter old notion, the speaker had found alluded to by Henry More in his philosophical "Conjectura Cabbalistica;" and in reference to that, and other works of the same period, in which creative forces and the nature and origin of animal species were treated of, the equal spirit, vigorous intellect, profound learning, and laborious research of such men as Cudworth, More, and Grew, contrasted most favourably with the opposite characteristics of some works of the same kind of the present day.

True it was that the old authors referred to exemplified the prevalent belief of their age in witches and apparitions. But has not our age its clairvoyants and spirit-rappers? Two centuries ago it was believed that a long round piece of wood, if bestridden by a person properly "possessed," would, under such influence, move against gravity, rise from the ground, and transport itself and its rider through the air. In our time it has been and may be still believed, that a flat circular piece of wood, on legs, can be made,

by the imposition of hands of believers, to move against gravity, tilt up, and dance about. We know that this belief had become so scandalously prevalent, as to make our excellent friend, the Fullerian Professor, deem it worth his while to deal it a crushing blow of his intellectual sledge-hammer.

The present age may be more knowing, but can it truly flatter itself as being wiser, more logical, and less credulous than that of Cudworth and More? As numerous and respectable eye-witnesses, in the seventeenth century, vouched for the transit of witches on broomsticks through the air; as, in the nineteenth, have testified to table-tilting and table-turning! And whether the occult influence over-ruling the law of gravitation charges a discoid or cylindrical substance—oozes from the palm of the hand, or emanates from another part of the body—matters not.

We ignore Satanic influences now-a-days, as a general rule. An impulse, said to be uncontrollable because uncontrolled, and a "black-cloud" over the mind, form grounds for acquitting the adulterous prolicide: of old, it would have been referred, in good earnest, to "the instigation of the devil," and an exemplary judgment would have followed the belief. It may be doubted whether the fearfully increasing number of acquittals, and of "merciful recommendations" of murderers, be a sign of the superior wisdom and humanity of our times.

But, returning to the sphere of that "lower wisdom, which rests in the contemplation of natural causes and dimensions," seeing that notions, refuted and repudiated centuries ago, are, in our day, revived and popularised, with a semblance of support from the later acquisitions of science, it is meet that they should be brought to the test of the exact results of modern inquiry: and this was the aim the Professor had in view in treating of the organical characters of the highest of the brute creation and their relations to those of the human species.

For about two centuries, naturalists have been cognizant of a small ape, tailless, without cheek-pouches, and without the ischial callosities, clothed with black hair, with a facial angle of about 60°, and of a physiognomy milder and more human-like than in the ordinary race of monkeys, less capricious, less impulsive in its habits, more staid and docile. This species, brought from the West Coast of Africa, is that which our anatomist, Tyson, dissected: he described the main features of its organisation in his work published in 1699. He called it the *Homo Sylvestris*, or pigmy. It is noted by Linnæus, in some editions of his *Systema Naturæ*, as the *Homo Troglodytes*. Blumenbach, giving a truer value to the condition of the innermost digit of the hind foot, which was like a thumb, called it the *Simia Troglodytes*; it afterwards became more commonly known as the "Chimpanzee."

At a later period, naturalists became acquainted with a similar kind of ape, of quiet docile disposition, with the same sad, human-

like expression of features. It was brought from Borneo or Sumatra; where it is known by the name of *Orang*, which, in the language of the natives of Borneo, signifies "man," with the distinctive addition of *Outan*, meaning "Wood-man," or "Wild Man of the Woods." This creature differed from the pigmy, or *Simia Troglodytes* of Africa, by being covered with hair of a reddish-brown colour, and by having the anterior, or upper limbs, much longer in proportion, and the thumb upon the hind feet somewhat less. It was entered in the zoological catalogue as the *Simia Satyrus*. A governor of Batavia, Baron Wurmb, had transmitted to Holland, in 1780, the skeleton of a large kind of ape, tailless, like this small species from Borneo, but with a much-developed face, and large canine teeth, and bearing thick callosities upon the cheeks, giving it, upon the whole, a very baboon-like physiognomy; and he called it the *Pongo*.

At the time when Cuvier revised his summary of our knowledge of the animal kingdom, in his second edition of the *Règne Animal*, 1829, the knowledge of the anthropoid apes was limited to these three forms. It had been suspected that the pongo might be the adult form of the orang; but Cuvier, pointing to its distinctive character, suggested that it could hardly be the same species. The facial angle of the small red orang of Borneo, and of the small black chimpanzee of Africa, brought them, from the predominant cranium, and small size of the jaws and small teeth, nearer than any other known mammalian animal to the human species, particularly to the lower, or negro forms. It was evident, from the examination of these small chimpanzees and orangs, that they were the immature of some large species of ape. The small size and number of their teeth, (there being in some of the smaller specimens, only twenty, like the number of deciduous teeth in the human species,) and the intervals between those teeth, all showed them to be of the first or deciduous series. Professor Owen had availed himself of the rich materials in regard to these animals collected about that time by the Zoological Society, to investigate the state of dentition, and the state of the teeth—the permanent teeth—that might be hidden in the substance of the jaws, of both the immature orang-outang and the chimpanzee, and had found, that the germs of those teeth in the orang-outang agreed in size with the permanent teeth that were developed in the jaws of a species of pongo of Wurmb, which Sir Stamford Raffles had presented to the museum of the College of Surgeons some years before. Specimens of orangs since acquired, of an intermediate age, have shown the progressive change of the dentition. The skull of one of these was exhibited, showing the huge anterior incisors co-existing with the small milk canines.

In the substance of the jaw are found the germs of the great canines, germs of large bicuspid teeth, showing the changes that must take place when the jaw is sufficiently enlarged to receive permanent teeth of this kind; and when the rest of the cranium is

modified, as it must be, concomitantly for the attachment of muscles to work the jaw so armed, that all these changes must result in the acquisition of characters such as are presented by the skulls of the large pongo, or Bornean baboon-like ape. The specific identity of the pongo, with certain of the young orang-outangs, was thus satisfactorily made out, and is now admitted by all naturalists. With regard to the chimpanzee, the germs of similarly proportioned large teeth were also discovered in the jaws, likewise indicating that it must be the young of a much larger species of ape.

The principal osteological characters of the chimpanzee and orang, commencing from the vertebral column, were as follows :—The vertebral column describes only one curve, inclining forwards, where it supports the head with its large jaws and teeth. The vertebræ in the neck, seven in number as usual in the mammalia, are chiefly remarkable for the great length of the simple spinous processes developed more than in most of the inferior apes, in relation to the necessities of the muscular masses that are to sustain and balance the head that preponderates so much forward on the neck. The vertebræ maintain a much closer correspondence in size, from the cervical to the dorsal and lumbar region, than in the human skeleton. With regard to the dorsal vertebræ, or those to which moveable ribs are articulated, there are twelve pairs in the orang; seven of them send cartilages to join the sternum, which is more like the sternum in man than in any of the inferior quadrumana : it is shorter and broader. In the smaller long-armed apes (*Hylobates*), which make the first step in the transition from the ordinary quadrumana to the man-like apes, the sternum is remarkably broad and short. The lumbar vertebræ are five in number in this adult orang. The sacrum is broader than in the lower quadrumana, but it is still narrow in comparison with its proportions in man. The pelvis is longer. The iliac bones are more expanded than in the lower quadrumana, but are more expanded on the same plane, and are flattened and long. The tuberosities of the ischia are remarkably developed, and project outwards. All these conditions of the vertebral column indicate an animal capable only of a semi-erect position, and present a modification of the trunk much better adapted for a creature destined for a life in trees, than one that is to walk habitually erect upon the surface of the ground. But that adaptation of the skeleton is still more strikingly shown in the unusual development of the upper prehensile extremities. The scapula is broad, with a well-developed spine and acromion; there is a complete clavicle; the bone of the arm (*humerus*) is of remarkable length, in proportion to the trunk; the radius and the ulna are also very long, and unusually diverging, to give increased surface of attachment to muscles; the hand is remarkable for the length of the metacarpus, and of the phalanges, which are slightly bent towards the palm; the thumb is less developed than the corresponding digit in the foot; the whole hand is admirably adapted

for retaining a firm grasp of the boughs of trees. In the structure of the carpus, there is a well-marked difference from the human subject, and a retention of the character met with in the lower quadrumana; the scaphoid bone being divided in the orang-outang. In the chimpanzee the bones of the carpus are eight, as in the human subject, but differ somewhat in form. If the upper extremities are so extraordinary for their disproportionate length, the lower ones are equally remarkable for their disproportionate smallness in comparison with the trunk, in the orang. The femur is short and straight, and the neck of the thigh-bone comparatively short. The head of the thigh-bone in this animal, which requires the use of these lower prehensile organs to grasp the branches of trees, and to move freely in many directions, is free from that ligament which strengthens the hip-joint in man; the head of the femur in the orang is quite smooth, without any indication of that attachment. Here, again, the chimpanzee manifests a nearer approach to man, for the *ligamentum teres* is present in it, in accordance with the stronger and better development of the whole hind-limb. This approximation, also, is more especially marked in the larger development of the innermost of the five digits of the foot in the chimpanzee, which is associated with a tendency to move more frequently upon the ground, to maintain a more erect position than the orang-outang, and to walk further without the assistance of a stick. The foot, in both these species of anthropoid orangs, is characterised by the backward position of the ankle-joint, presented by the astragalus to the tibia, which serves for the transference of the superincumbent weight upon the foot; by the comparatively feeble development of the backward projecting process of the calcaneum; by the obliquity of the articular surface of the astragalus, which tends to incline the foot a little inwards, taking away from the plantigrade character of the creatures and from their capacity to support themselves in an erect position, and giving them an equivalent power of applying their prehensible feet to the branches of the trees in which they live.

With reference to the chimpanzee, it was further observed, that, although the number of the true vertebræ is the same as in the orang, yet there is an additional pair of ribs developed; but, as there are thirteen dorsal vertebræ, we find only four lumbar ones. The modifications of the pelvis are a close repetition of those of the orang-outang. The chief differences in the skeleton of the chimpanzee are, a shortening and strengthening of the upper extremities, an approach towards the characteristic proportions of those parts in man; the presence of the ligamentum teres of the hip-joint; and the greater development of the innermost toe in the foot. In both the orang and chimpanzee the skull is articulated by condyles, which are placed far back on its under surface. The cranium is small, characterised by well-developed occipital and sagittal ridges; the occipital ridges in reference to the muscles sustaining

the head; and the sagittal ones in reference to an increased extent of the temporal muscles. The zygomatic arches are strong, and well arched outwards. The lower jaw is of great depth, and has powerful ascending rami, but the chin is wanting. The facial angle is about 50° to 55° in the full-grown *simia satyrus*, and 55° to 60° in the *troglodytes niger*. The difference in the facial angle between the young and adult apes, (which, in the young chimpanzee, approaches 60° to 65°,) depends upon those changes consequent upon the shedding of the deciduous teeth and the concomitant development of the jaws and intermuscular processes of the cranium.

But the knowledge of the species of these anthropoid apes has been further increased since the acquisition of a distinct and precise cognisance of the characters of the adults of the orang and chimpanzee. First, in reference to the orangs of Borneo, specimens have reached this country which show that there is a smaller species in that island, the *Simia Morio*, in which the canines are less developed, in which the bony *cristæ* are never raised above the level of the ordinary convexity of the cranium, and in which the callosities upon the cheeks are absent, associated with other characteristics plainly indicating a specific distinction. The Rajah Brooke has confirmed the fact of the existence in the island of Borneo of two distinct species of red orangs; one of a smaller size and somewhat more anthropoid; and the larger species presenting the baboon-like cranium.

In reference to the black chimpanzee of Africa also, another very important addition has been made to our knowledge of those forms of highly developed quadrumana. In 1847 Professor Owen received a letter from Dr. Savage, a church-missionary at Gaboon, enclosing sketches of the crania of an ape, which he described as much larger than the chimpanzee, ferocious in its habits, and dreaded by the negro natives more than they dread the lion or any other wild beast of the forest. These sketches showed plainly one cranial characteristic by which the chimpanzee differs in a marked degree from the orangs; viz. that produced by the prominence of the supra-orbital ridge, which is wanting in the adult and immature of the orangs. That ridge was strongly marked in the sketches transmitted. At a later period in the same year, Mr. Stuchbury transmitted to the Professor from Bristol two skulls of the same large species of chimpanzee, received from the same locality in Africa, bringing clearly to light evidence of the existence in Africa of a second larger and more powerful ape,—the *troglodytes gorilla*. This species presents the characters of the cranium which are seen in the immense development of the occipital and parietal cristæ in relation to the muscles of the neck and jaws,—repeating, and even exaggerating, the prominence of the supra-orbital ridges of the chimpanzee, and showing the same characteristic inequality in the development of the teeth, particularly in the exaggerated size of the canine teeth, as in the great pongo of Wurmb. But the gorilla differs from the orang, and resembles man, first in the minor develop-

ment of the intermaxillary bone—that part being more produced in the orang-outang, giving it a less open facial angle; it differs also in the form of the orbits, which in the orang-outang are a full oval, but in the gorilla square-shaped, more nearly approaching those in the human species; it differs likewise in significant modifications of the base of the skull, as, for example, in the greater depth of the glenoid cavity, which joint, instead of being defended by merely the post-glenoid process in the inferior quadrumana, is now defended by a ridge developed from the tympanic bone, which ridge corresponds with the vaginal process in the human subject. In the conformation of the grinding surface of the teeth there is also a marked difference, which brings this great ape of Africa more nearly to the human subject, in reference to that character, than any other known species of the quadrumanous order. The characteristic of the grinding surface of the molar teeth in man, as may be seen in almost all varieties of the human species, is, that the four tubercles on the grinding surface are united by a ridge describing a strong sigmoid curvature. This is repeated in the gorilla, which also presents another important approach to the human subject in the commencement of a projection of the nasal bone. Like the chimpanzee, the superior extremities are longer than the lower ones; and the innermost digit of the foot is converted into a powerful thumb.

Having premised this account of the mature characters of the different species of orangs and chimpanzees, the lecturer next proceeded to contrast their structure with that of man. With regard to the dentition of these anthropoid apes, the number and kinds of the teeth, like those of all the quadrumana of the old world, correspond with those in the human subject; but all these apes differ in the larger proportionate size of the canine teeth, which necessitates a certain break in the series, in order that the prolonged points of the canine teeth may pass into their place when the mouth is completely closed. In addition to the larger proportionate size of the incisors and canines, the bicuspids in both jaws are implanted by three distinct fangs—two external and one internal: in the human species, the bicuspids are implanted by one external and one internal fang: in the highest races of man these two fangs are often connate; very rarely is the external fang divided, as it constantly is in all the species of the orang and the chimpanzee.

With regard to the catarrhine, or old-world quadrumana, the number of milk teeth is twenty, as in the human subject. But both chimpanzees and orangs differ from man in the order of development of the permanent series of teeth: the second true molar comes into place before either of the bicuspids have cut the gum, and the last molar is acquired before the permanent canine. We may well suppose that the larger grinders are earlier required by the frugivorous apes than by the omnivorous human race; and one condition of the earlier development of the canines and *bicuspids in man*, may be their smaller relative size as compared

with the apes. The great difference is the predominant development of the permanent canine teeth, at least in the males of the orangs and chimpanzees; for this is a sexual distinction, the canines in the females never presenting the same large proportion. In man, the dental system, although the formula is the same as in the apes, is peculiar for the equal length of the teeth, arranged in an uninterrupted series, and shows no sexual distinctions. The characteristics of man are exhibited in a still more important degree in the parts of the skeleton. His whole framework proclaims his destiny to carry himself erect; the anterior extremities are liberated from any service in the mere act of locomotion, and are perfected to be the fitting instruments of the rational mind and free will with which he is endowed. The speaker proceeded to trace these modifications from the foundation upwards.

With regard to the foot, it had been shown in a former discourse "On the Nature of Limbs," that in tracing the manifold and progressive changes of the feet in the mammalian series, in those forms where it is normally composed of five digits, the middle is usually the largest; and this is the most constant one. The modifications in the hind foot, in reference to the number of digits, are, first, the reduction and then the removal, of the innermost one; then the corresponding reduction and removal of the outer one; next, of the second and fourth digits, until it is reduced to the middle digit, as in the horse.

The innermost toe, the first to dwindle and disappear in the brute series, is, in man, developed to a maximum of size, becoming emphatically the "great toe," one of the most essential characteristics of the modifications of the human frame. It is made the powerful fulcrum for that lever of the second kind, which has its resistance in the tibio-astragalar joint, and the power applied to the projecting heel-bone: the superincumbent weight is carried further forwards upon the foot, by the more advanced position of the astragalus, than in the ape tribe; and the heel-bone is much stronger, and projects more backwards.

The arrangement of the powerfully-developed tarsal and metatarsal bones is such as to form a bony arch, of which the two piers rest upon the proximal joint of the great toe and the end of the heel. Well-developed cuneiform bones combine with the cuboid to form a second arch, transverse to the first. There are no such modifications in the orangs, in which the arch, or rather the bend of the long and narrow sole, extends to the extreme end of the long and curved digits, indicating a capacity for grasping. Upon these two arches the superincumbent weight of man is solidly and sufficiently maintained, as upon a low dome, with this further advantage, that the different joints, cartilages, coverings, and synovial membranes, give a certain elasticity to the dome, so that in leaping, running, or dropping from a height, the jar is diffused and broken before it can be transmitted to affect the enormous

semi-expanded cranium. The hind limbs in man are longer in proportion to the trunk than in any other known mammalian animal. The kangaroo might seem to be an exception, but if the hind limbs of the kangaroo are measured in relation to the trunk, they are shorter than in the human subject. In no animal is the femur so long in proportion to the leg as in man. In none does the tibia expand so much at its upper end. Here it presents two broad, shallow cavities, for the reception of the condyles of the femur. Of these condyles, in man only is the innermost longer than the outermost; so that the shaft of the bone inclines a little outwards to its upper end, and joins a "neck" longer than in other animals, and set on at a very open angle. The weight of the body, received by the round heads of the thigh bones, is thus transferred to a broader base, and its support in the upright posture facilitated. There is also the collateral advantage of giving more space to those powerful adductor muscles that assist in fixing the pelvis and trunk upon the hind limbs. With regard to the form of the pelvis, you could not fully appreciate its peculiar modifications unless you saw it, as here displayed, in contradistinction to the form of the pelvis in the highest organised quadrumana. The short and broad ilium bends forwards, the better to receive and sustain the abdominal viscera, and is expanded behind to give adequate attachment to the powerful glutei muscles, which are developed to a maximum in the human species, in order to give a firm hold of the trunk upon the limbs, and a corresponding power of moving the limbs upon the trunk. The tuberosities of the ischium are rounded, not angular, and not inclined outwards, as in the ape tribe. The symphysis pubis is shorter than in the apes. The tail is reduced to three or four stunted vertebræ, anchylosed to form the bone called "os coccygis." The true vertebræ, as they are called in human anatomy, correspond in number with those of the chimpanzee and the orang, and in their divisions with the latter species, there being twelve thoracic, five lumbar, and seven cervical. This movable part of the column is distinguished by a beautiful series of sigmoid curves, convex forwards in the loins, concave in the back, and again slightly convex forwards in the neck. The cervical vertebræ, instead of having long spinous processes, have short processes, usually more or less bifurcated. The bodies of the true vertebræ increase in size from the upper dorsal to the last lumbar, which rests upon the base of the broad wedge-shaped sacrum, fixed obliquely between the sacro-iliac articulations. All these curves of the vertebral column, and the interposed elastic cushions, have relation to the libration of the head and upper limbs, and the diffusion and the prevention of the ill effects from shocks in many modes of locomotion which man, thus organised for an erect position, is capable of performing. The arms of man are brought into more symmetrical proportions with the lower limbs; and their bony framework shows all the perfections that have been superinduced upon it in the mammalian

series, viz., a complete clavicle, the antibrachial bones so adjusted as to permit the rotatory movements of pronation and supination, as well as of flexion and extension; manifesting those characters which adapt them for the manifold application of that most perfect and beautiful of prehensile instruments, the hand. The scapula is broad, with the glenoid articulation turned outwards; the clavicles are bent in a slight sigmoid flexure; the humerus exceeds in length the bones of the fore-arm. The carpal bones are eight in number. The thumb is developed far beyond any degree exhibited by the highest quadrumana, and is the most perfect opposing digit in the animal creation. The skull is distinguished by the enormous expansion of the brain case; by the restricted growth of the bones of the face, especially of the jaws, in relation to the small, equally-developed teeth; and by the early obliteration of the maxillo-intermaxillary suture. To balance the head upon the neck-bone, we find the condyles of the occiput brought forward almost to the centre of the base of the skull, resting upon the two cups of the atlas, so that there is but a slight tendency to incline forwards when the balancing action of the muscle ceases, as when the head nods during sleep, in an upright posture. Instead of the strongly developed occipital crest, we find a great development of true mastoid processes advanced nearer to the middle of the sides of the basis cranii, and of which there is only the rudiment in the gorilla. The upper convexity of the cranium is not interrupted by any sagittal or parietal cristæ. The departure from the archetype, in the human skull, is most conspicuous, in the vast expanse of the neural spines of the three chief cranial vertebræ, viz., occipital, parietal, and frontal.

The Professor next entered upon the question, "To what extent does man depart from the typical character of his species?" With regard to the kind and amount of variety in mankind, we find, propagable and characteristic of race, a difference of stature, a difference in regard to colour, difference in both colour and texture of the hair, and certain differences in the osseous framework. With regard to stature, the Bushmen of South Africa and the natives of Lapland exhibit the extreme of diminution, ranging from four to five feet. Some of the Germanic races and the Patagonian Indians exhibit the opposite extreme, ranging from six to seven feet. The medium size prevails generally throughout the races of mankind. With reference to the characteristics of colour, which are extreme, we have now opportunities of knowing how much that character is the result of the influence of climate. We know it more particularly by that most valuable mode of testing such influences which we have from the peculiarity of the Jewish race. For 1800 years that race has been dispersed into different latitudes and climates, and they have preserved themselves most distinct from any intermixture with the other races of mankind. There are some Jews still lingering in the valleys of the Jordan, having been oppressed

by the successive conquerors of Syria for ages,—a low race of people, and described by trustworthy travellers as being as black as any of the Ethiopian races. Others of the Jewish people, participating in European civilization, and dwelling in the northern nations, show instances of the light complexion, blue eyes, and light hair of the Scandinavian families. The condition of the Hebrews, since their dispersion, has not been such as to admit of much admixture by the proselytism of household slaves. We see, then, how to account for the differences in colour, without having to refer them to original or specific distinctions. As to the difference in size in mankind, it is slight in comparison with what we observe in the races of the domestic dog, where the extremes of size are much greater than can be found in any races of the human species. With reference to the modifications of the bony structure, as characteristic of the races of mankind, they are almost confined to the pelvis and the cranium. In the pelvis the difference is a slight, yet apparently a constant one. The pelvis of the adult negro may sometimes be distinguished from that of the European by the greater proportional length and less proportional breadth of the iliac bones; but how trifling is this difference compared with that marked distinction in the pelvis which the orang-outang presents!

With regard to the cranial differences, the Professor selected for comparison three extreme specimens of skulls characteristic of race: one of an aboriginal of Van Diemen's Land (the lowest of the Melanian or dark-coloured family), a well-marked Mongolian, and a well-formed European skull. The differences were described to be chiefly these. In the low, uneducated, uncivilised races, the brain is smaller than in the higher, more civilised, and more educated races; consequently the cranium rises and expands in a less degree. Concomitant with this contraction of the brain-case is a greater projection of the fore-part of the face; whether it may be from a longer exercise of the practice of suckling, or a more habitual application of the teeth in the inter-maxillary part of the jaw, and in the corresponding part of the lower jaw, in biting and gnawing tough, raw, uncooked substances,—the anterior alveolar part of the jaws does project more in those lower races; but still to an insignificant degree compared with the prominence of that part of the skull in the large apes. And while alluding to them, the speaker again adverted to the distinction between them and the lowest of the human races, which is afforded by the inter-maxillary bone, already referred to. In the young orang-outang, even when the change of dentition has begun, the suture between that bone and the maxillary is present; and it is not until the large canine teeth are developed, that the stimulus of the vascular system, in the concomitant expansion and growth of the alveoli, tends to obliterate the suture. In the young chimpanzee, the maxillary suture disappears earlier, at least on the facial surface of the upper jaw. In

the human subject those traces disappear still earlier, and in regard to the exterior alveolar plates, the inter-maxillary and maxillary bones are connate. But there may be always traced in the human fœtus the indications of the palatal and nasal portions of the maxillo-intermaxillary suture, of which the poet Goëthe was the first to appreciate the full significance.

In the Mongolian skull there is a peculiar development of the cheek-bones, giving great breadth and flatness to the face, a broad cranium, with a low forehead, and often with the sides sloping away from the median sagittal tract, something like a roof; whereas, in the European, there is combined, with greater capacity of the cranium, a more regular and beautiful oval form, a loftier and more expanded brow, a minor prominence of the malars, and a less projection of the upper and lower jaws. All these characteristics necessarily occasion slight differences in the facial angle. On a comparison of the basis cranii, the strictly bimanous characteristics in the position of the foramen magnum and occipital condyles, and of the zygomatic arches, are as well displayed in the lowest as in the highest varieties of the human species.

With regard to the value to be assigned to the above defined distinctions of race:—in consequence of not any of these differences being equivalent to those characteristics of the skeleton, or other parts of the frame, upon which specific differences are founded by naturalists in reference to the rest of the animal creation, the Professor came to the conclusion that man forms one species, and that these differences are but indicative of varieties. As to the number of these varieties:—from the very well marked and natural character of the species, just as in the case of the similarly natural and circumscribed class of birds, scarcely any two ethnologists agree as to number of the divisions, or as to the characters upon which those varieties are to be defined and circumscribed. In the subdivision of the class of birds, the ornithological systems vary from two orders to thirty orders; so with man there are classifications of races varying from *thirty* to the *three* predominant ones which Blumenbach first clearly pointed out,—the Ethiopian, the Mongolian, and the Caucasian or Indo-European. These varieties merge into one another by easy gradations. The Malay and the Polynesian link the Mongolian and the Indian varieties; and the Indian is linked by the Esquimaux again to the Mongolian. The inhabitants of the Andaman Islands, New Caledonia, New Guinea, and Australia, in a minor degree seem to fill up the hiatus between the Malay and the Ethiopian varieties; and in no case can a well marked definite line be drawn between the physical characteristics of allied varieties, these merging more or less gradationally the one into the other.

In considering the import and value of the osteological differences between the gorilla—the most anthropoid of all known brutes—and man, in reference to the hypothesis of the origination of

species of animals by gradual transmutation of specific characters, and that in the ascending direction, Professor Owen admitted that the skeleton may be modified to a certain extent by the action of the muscles to which it is subservient, and that in domesticated races the size of the animal may be brought to deviate in both directions from the specific standard. By the development of the processes, ridges, and crests, and also by the general proportions of the bones themselves, especially those of the limbs, the human anatomist judges of the muscular power of the individual to whom a skeleton under comparison has appertained.

The influence of muscular actions in the growth of bone is more strikingly displayed in the change of form which the cranium of the young carnivore or the sternum of the young bird undergoes in the progress to maturity; not more so, however, than is manifested in the progress of the development of the cranium of the gorilla itself, which results in a change of character so great, as almost to be called a metamorphosis.

In some of the races of the domestic dog, the tendency to the development of parietal and occipital cristæ is lost, and the cranial dome continues smooth and round from one generation of the smaller spaniel, or dwarf pug, *e. g.* to another; while, in the large deer-hound, those bony cristæ are as strongly developed as in the wolf. Such modifications, however, are unaccompanied by any change in the connexions, that is, in the disposition of the sutures of the cranial bones; they are due chiefly to arrests of development, to retention of more or less of the characters of immaturity: even the large proportional size of the brain in the smaller varieties of house-dog is in a great degree due to the rapid acquisition by the cerebral organ of its specific size, agreeably with the general law of its development, but which is attended in the varieties cited by an arrest of the general growth of the body, as well as of the particular developments of the skull in relation to the muscles of the jaws.

No species of animal has been subject to such decisive experiments, continued through so many generations, as to the influence of different degrees of exercise of the muscular system, difference in regard to food, association with man, and the concomitant stimulus to the development of intelligence, as the dog; and no domestic animal manifests so great a range of variety in regard to general size, to the colour and character of the hair, and to the form of the head, as it is affected by different proportions of the cranium and face, and by the intermuscular crests superadded to the cranial parietes. Yet, under the extremest mask of variety so superinduced, the naturalist detects in the dental formula and in the construction of the cranium the unmistakeable generic and specific characters of the *canis familiaris*. This and every other analogy applicable to the present question justifies the conclusion that the range of variety allotted to the chimpanzee under the operation of external circum-

stances favourable to its higher development would be restricted to differences of size, of colour, and other characters of the hair, and of the shape of the head, in so far as this is influenced by the arrest of general growth after the acquisition by the brain of its mature proportions, and by the development, or otherwise, of processes, crests, and ridges for the attachment of muscles. The most striking deviations from the form of the human cranium which that part presents in the great orangs and chimpanzees result from the latter acknowledged modifiable characters, and might be similarly produced; but not every deviation from the cranial structure of man, nor any of the important ones upon which the naturalist relies for the determination of the genera *troglodytes* and *pithecus*, have such an origin or dependent relation. The great chimpanzee, indeed, differs specifically from both the orang and man in one cranial character, which no difference of diet, habit, or muscular exertion can be conceived to affect.

The prominent superorbital ridge, for example, is not the consequence or concomitant of muscular development; there are no muscles attached to it that could have excited its growth. It is a characteristic of the cranium of the genus *troglodytes* from the time of birth to extreme old age; by the prominent superorbital ridge, for example, the skull of the young chimpanzee with deciduous teeth may be distinguished at a glance from the skull of an orang at the same immature age; the genus *pithecus*, Geoffr., being as well recognised by the absence, as the genus *troglodytes* is by the presence, of this character. We have no grounds, from observation or experiment, to believe the absence or the presence of a prominent superorbital ridge to be a modifiable character, or one to be gained or lost through the operations of external causes, inducing particular habits through successive generations of a species. It may be concluded, therefore, that such feeble indication of the superorbital ridge, aided by the expansion of the frontal sinuses, as exists in man, is as much a specific peculiarity of the human skull, in the present comparison, as the exaggeration of this ridge is characteristic of the chimpanzees and its suppression of the orangs.

The equable length of the human teeth, and the concomitant absence of any diastema or break in the series, and of any sexual difference in the development of particular teeth, are to be viewed by the light of actual knowledge, as being primitive and unalterable specific peculiarities of man.

Teeth, at least such as consist of the ordinary dentine of mammals, are not organised so as to be influenced in their growth by the action of neighbouring muscles; pressure upon their bony sockets may affect the direction of their growth after they are protruded, but not the specific proportions and forms of the crowns of teeth of limited and determinate growth. The crown of the great canine tooth of the male *troglodytes gorilla* began to be calcified when its diet was precisely the same as in the female, when both

sexes derived their sustenance from the mother's milk. Its growth proceeded and was almost completed before the sexual development had advanced so as to establish those differences of habits, of force, of muscular exercises, which afterwards characterise the two sexes. The whole crown of the great canine is, in fact, calcified before it cuts the gum or displaces its small deciduous predecessor; the weapon is prepared prior to the development of the forces by which it is to be wielded; it is therefore a structure fore-ordained, a predetermined character of the chimpanzee, by which it is made physically superior to man; and one can as little conceive its development to be a result of external stimulus, or as being influenced by the muscular actions, as the development of the stomach, the testes, or the ovaria.

The two external divergent fangs of the premolar teeth, and the slighter modifications of the crowns of the molars and premolars, appear likewise from the actual results of observation to be equally predetermined and non-modifiable characters.

No known cause of change productive of varieties of mammalian species could operate in altering the size, the shape, or the connexions of the premaxillary bones, which so remarkably distinguish the great *troglodytes gorilla*, not from man only, but from all other anthropoid apes. We know as little the conditions which protract the period of the obliteration of the sutures of the premaxillary bones in the *tr. gorilla* beyond the period at which they disappear in the *tr. niger*, as we do those that cause them to disappear in man earlier than they do even in the smaller species of chimpanzee.

There is not, in fact, any other character than those founded upon the developments of bone for the attachment of muscles, which is known to be subject to change through the operation of external causes; nine-tenths, therefore, of the differences, especially those very striking ones manifested by the pelvis and pelvic extremities, which the Professor had cited in memoirs on the subject, published in the "Zoological Transactions," as distinguishing the great chimpanzee from the human species, must stand in contravention of the hypothesis of transmutation and progressive development, until the supporters of that hypothesis are enabled to adduce the facts and cases which demonstrate the conditions of the modifications of such characters.

If the consideration of the cranial and dental characters of the *troglodytes gorilla* has led legitimately to the conclusion that it is specifically distinct from the *troglodytes niger*, the hiatus is still greater that divides it from the human species, between the extremest varieties of which there is no osteological and dental distinction which can be compared to that manifested by the shorter premaxillaries and larger incisors of the *troglodytes niger* as compared with the *tr. gorilla*.

The analogy which the establishment of the second and more formidable species of chimpanzee in Africa has brought to light

between the representation of the genus *troglodytes* in that continent, and that of the genus *pithecus* in the great islands of the Indian Archipelago, is very close and interesting. As the *troglodytes gorilla* parallels the *pithecus Wurmbii*, so the *troglodytes niger* parallels the *pithecus morio*, and an unexpected illustration has thus been gained of the soundness of the interpretation of the specific distinction of that smaller and more anthropoid orang.

It is not without interest to observe, that as the generic forms of the *quadrumana* approach the *bimanous* order, they are represented by fewer species. The gibbons (*hylobates*) scarcely number more than half-a-dozen species; the orangs (*pithecus*) have but two species, or at most three; the chimpanzees (*troglodytes*) are represented by two species.

The unity of the human species is demonstrated by the constancy of those osteological and dental characters to which the attention is more particularly directed in the investigation of the corresponding characters in the higher *quadrumana*.

Man is the sole species of his genus, the sole representative of his order; he has no nearer physical relations with the brute-kind than those which flow from the characters that link together the primary (unguiculate) division of the placental sub-class of mammalia.

Professor Owen trusted that he had furnished the confutation of the notion of a transformation of the ape into man, which had been anticipated by the old author to whom he had referred at the outset, and strongly recommending his writings to those of his hearers who might not be acquainted with them, he concluded by quoting the passage referred to.

"And of a truth, vile epicurism and sensuality will make the soul of man so degenerate and blind, that he will not only be content to slide into brutish immorality, but please himself in this very opinion that he is a real brute already, an ape, satyre or baboon; and that the best of men are no better, saving that civilizing of them and industrious education has made them appear in a more refined shape, and long inculcate precepts have been mistaken for connate principles of honesty and natural knowledge; otherwise there be no indispensable grounds of religion and vertue, but what has hapned to be taken up by *over-ruling* custom. Which things, I dare say, are as easily confutable, as any conclusion in mathematics is demonstrable. But as many as are thus sottish, let them enjoy their own wildness and ignorance; it is sufficient for a good man that he is conscious unto himself that he is more nobly descended, better bred and born, and more skilfully taught by the purged faculties of his own minde."*

[R. O.]

* Henry More's "Conjectura Cabbalistica," fol. (1662)—p. 175.

WEEKLY EVENING MEETING,

Friday, February 16.

Frederick Pollock, Esq. M.A. in the Chair.

Edward Jekyll, Esq. M.R.I.

On Siege Operations.

The speaker, after a few preliminary observations, commenced by stating, that it is absolutely necessary for a besieging army thoroughly to invest the place about to be attacked; that is, simultaneously to occupy positions so as to cut off all communication with the threatened fortress, and to have a numerical force seven or eight times the number of the pent-up garrison. A reconnoissance is then made by the engineers, who, during the first part of the investment, are employed in taking notes of the description of the different fronts of the fortification, in making a correct plan of the work, and the ground in its vicinity; in which the course of rivers, streams, ravines, and roads, the extent of possible inundations, woods, marshes, or eminences, are accurately laid down. They mark out, with great precision, by means of pickets, placed in the ground, the prolongation of all the faces of the most prominent works, and the salient angles as well: not only because the latter are the shortest road to the fortress, but because they are also the paths the least exposed to the enemy's fire.

During this reconnoissance, the besieging army, having encamped out of range of the guns of the place, send forth large working parties to cut down all the timber and brushwood in the neighbourhood, wherewith to construct the necessary materials for the siege. These consist of gun platforms, timber for the lining and support of mine shafts, galleries, and magazines; but more particularly for the making of gabions, sap-rollers, and fascines.

The *gabion* is a cylindrical basket of wicker work, open at both ends, and of various dimensions, but usually from three to four feet in height, and three feet in diameter. Its use and object being to construct hastily a shot-proof breastwork or parapet, when filled with earth, or to line the approaches and batteries when the soil is of a loose and crumbling nature.

The *sap-roller* consists of two concentric gabions, placed one within the other, each six feet long, the interval between them being stuffed with logs of hard wood; the whole mass far exceeding

the dimensions of the ordinary gabion. It is employed to protect the sapper engaged at the head of an approach or trench, when advancing such work towards the enemy.

The *fascine* is a faggot of brushwood, eighteen feet in length, and nine inches in diameter; its use being to line the parapets, and various earthworks constructed during the progress of the siege.

Bags filled with earth are also prepared, and largely employed during the operations; the whole are then stored in that part of the camp called the Engineers' Park. The number of these materials is enormous, and the following estimate often has to be exceeded, or even doubled, namely, 80,000 gabions, 100,000 fascines, 120,000 sand bags, together with 4000 spades and shovels, and 3000 pick-axes, with other tools in like proportion.

The enemy having been kept in ignorance of the front of the fortress about to be attacked, and all the necessary arrangements having been made, let us examine the object of the assailant, and the manner in which he may best proceed to effect it. His endeavour is to possess himself of a fortress; and having seven or eight times as many troops as are shut up in the work, it follows that the larger number will overpower the weaker, if brought to a close combat; but the battle-field of the foe is so organized as to prevent such collision, surrounded as it is by obstructions which the assailants must overcome: the besieger is, therefore, compelled to use both industrious and scientific means, in making his attack, requiring more or less time in their completion, in proportion to the defences of the place, its strength, and the courage of its protectors.

The means employed since the invention of artillery, consist in choosing the front to be attacked, checking its fire, and in making a safe road by which the besieger can advance unseen to the foot of the ramparts; and lastly, in placing in well protected batteries his artillery to subdue the place and effect a breach in the walls of the fortress.

The first operation of the besieger is, to approach secretly by night with a working party of 1800 men, each carrying a fascine, pick-axe, and shovel, accompanied by an armed and protecting force equal to cope with the garrison; the former dig a trench 2000 yards in length, parallel to the fortifications attacked, (the direction having been previously marked out by the engineers,) and with the earth excavated from such trench, raise a bank or breastwork on the side nearest to the enemy; while the armed party, formed in a recumbent posture, remain in readiness to protect the workmen, should the garrison sally forth to attack them. During the night and following day the besiegers remain in the trench, till sufficient cover is gained to protect from the fire of the fortress all engaged, whether workmen or their appointed guard; but as each fifty men have a certain task allotted to them, they are relieved by a like number at the expiration of their labour.

This work, called the *first parallel*, is an envelope equi-distant from all the salient angles of the fortress, and it is along this road that all guns, men, and munitions can securely move, sheltered from the view and projectiles of the enemy. Batteries are then formed on the side next the place attacked, and a secure communication, made in like manner, is constructed towards the camp and entrepot of the besiegers.

The garrison having now discovered the front of their work about to be attacked, do all in their power to add to their defences; a double line of palisades is placed in the covered way; traverses are erected to lessen the effect of the enfilade and ricochet fire of the besiegers; the country on the side attacked is inundated, if such means exist; fresh embrasures are opened on the ramparts, and splinter proofs to prevent the ravages of shells are placed over the guns; safe communications are formed, leading to the outworks; mine galleries driven under the glacis (if none had been previously prepared,) and every means taken for repelling the advances of the besiegers. The fire from the guns, howitzers, and mortars of the assailants, is of a four-fold character: direct, to batter down such parts of the fortress as are not covered by the outworks; enfilade, to rake; ricochet, to bound down the faces of the ramparts, and dismount or otherwise injure the artillery; and vertical, or that from mortars, to destroy the storehouses, magazines, barracks, or depôts, within the walls of the place.

After some days' fire, the same species of covered road is carried forward from the first parallel, by certain rules of art, to approach the fortress; this trench proceeds in a zigzag direction, crossing and re-crossing the direct line leading to the salient angle of the fortress, care being taken that its direction is such, that no fire from the enemy can rake or enfilade it. And at a distance of 300 yards from the works of the besieged, a new place of arms, or second parallel, is constructed similar to the first, wherefrom the assailants can support the head of their attack. New batteries are here formed, to further enfilade the threatened works, and also to counter-batter such collateral works of the defenders as contribute to the defence of the place, and the fire of which it is necessary to subdue. The assailant again advances by similar zigzags, till within 150 yards of the covered way of the enemy, where fresh lodgments, called the *demi-parallels*, are effected.

And here an entirely new feature in the attack presents itself: it being needful to keep down the heavy fire of riflemen, and wall pieces (heavy muskets fired from rests upon the parapets), and also to prevent workmen from repairing the injured defences, pierriers, or stone mortars, are placed in the wings of the aforesaid demi-parallels, which keep up an incessant discharge of large stones, 4-pound iron balls, and grenades, upon the front attacked. Volleys of such missiles are directed upon the shattered parapets, driving the defenders from the walls, and forcing them to fly to

places of cover and security, protecting themselves from these projectiles by such temporary buildings as they can erect. The enemy in reply keep up a continuous fire from small mortars, called royals and coehorns, upon the head of the advancing trench; light balls (a brilliantly burning firework), thrown by the garrison, disclose the operations of the enemy, who try to extinguish them with sand or wetted hides, and if such means fail, place smoke balls to obscure the light.

The approaches are now carried forward by sapping,—a most hazardous duty. The foremost workman, protected by the sap-roller, pushed in front by a long fork, places a gabion on the side nearest the fortress; he rapidly fills it with earth from the trench he is excavating (a labour he performs on his knees), digging the earth eighteen inches deep, and a like width, but never exposing himself beyond the first placed gabion. He is followed by three comrades, who increase the dimensions of the trench, and frequently relieve him in his perilous undertaking; sand-bags are placed in the hollows between each gabion, and thus safe cover is effected; ten feet of sap may be made in one hour. At the late siege of Antwerp, the French sappers were protected by helmets and cuirasses, their weight however impeded the movements of the men; and the celerity of the operation.

At this period of the siege, the fire from the place being much weakened, many guns dismounted, and the ramparts ploughed up by the severity of the besiegers' fire, a third parallel is at length formed at the foot of the glacis, and an attempt made to gain the covered way, the palisades in which have been broken and destroyed by the ricochet batteries. If this is to be effected by assault, the interior of the breastwork of the third parallel is made in steps, so that the assailants may simultaneously sally forth to attain their object: but the slower and more certain method is by the sap and mine. At the siege of Cambray, Dumetz stormed a work during the attack contrary to the advice of Vauban, and sustained a defeat, together with a loss of 40 officers, and 400 men; Vauban gained the same object two days later by sap, and lost but three lives.

The covered way being now in possession of the besiegers, breaching batteries to destroy the revetments of the fortress are constructed. The fire of six 24-pounders, so directed as to make perpendicular cuts in the masonry, play upon the wall: one long horizontal fissure three feet in depth is also effected, and by the firing in salvos or volleys, the loosened mass and superincumbent parapet falls bodily into the ditch, presenting a slope or means of ascent more or less practicable. The troops are led to the assault by means of a subterraneous gallery leading from the trenches to the ditch.

The garrison now usually capitulates. But if the latter part of the operations are carried on by the system of mining, the entire

character of the attack is changed; and as the besieger proceeds with the trenches on the surface of the ground he has to secure himself from below. Twelve days are added to the duration of the siege, if the fortress is ably protected by a well-arranged plan of defensive mines, in the more advanced galleries of which he can listen for the stroke of the miner's pick, and by means of a pea, placed upon a tightly-braced drum, subterraneous workmen can be discovered at the distance of from 60 to 90 feet in ordinary soil, hence such listening galleries, as they are termed, are built distant from each other 120 feet. When the advancing miner is discovered by the defenders of the fortress, a mine is hastily prepared, and the assailant blown to destruction. Occasionally a long iron probe is used, to ascertain the nature of the ground in front, or the position of the works of the besieged; and if such instrument reaches into the defensive excavations it is followed on withdrawal by a charged rifle or musquetoon, and a shot is fired upon the assailant, or combustibles generating noxious gases are thrust into the aperture.

The subject of mining is far too extensive a one to be embraced in so limited a description, but the globes of compression of the besiegers, or surcharged mines, finally overthrow the network of galleries with which the fortress is surrounded; and the craters or hollows formed by their explosion, afford cover and the more ready means of pushing forward the saps and trenches, and the fortress is compelled to surrender. In describing the various engines of war, and the recent improvements made in them, Mr. Jekyll alluded to the making of cannon shot of a conoidal form, and the recently discovered danger of exposing live shells to the enemies' fire, both in batteries and on ship-board;—shells struck by shot instantly explode, the blow raising the temperature of the stricken part far beyond the heat at which gunpowder inflames. Some of our first-rate men of war have their lower batteries of shell guns only; and as each gun has two shells in boxes placed over each piece of ordnance, 64 mines are thus prepared for the destruction of the vessel, liable, during action, to add their ravages to those occasioned by the fire of the foe.

In conclusion, comparison was drawn between the attack upon an ordinary pentagon, and the siege now in progress in the Crimea. In the former the prize was sure of being gained, inasmuch as the place was always previously invested, contained a garrison of but 5000 men, and was defended by 150 pieces of artillery, a portion only of which could be used in the defence of the single side attacked, a length seldom exceeding 320 yards; the besiegers, with an overwhelming force of men and ordnance, having established themselves behind safe approaches, batteries, and a parallel or envelope embracing the fortress of a length of 2000 yards, finally ruined the defences of the fortress. At Sevastopol investment had been impracticable; the parallel of the allies, broken by the nature of the ground, was of no greater extent than 2300 yards, and the Rus-

sian defences opposed a length little short of four miles, mounting 800 guns to the 500 of the combined armies, and aided by a garrison whose numbers were unknown and capable of continual augmentation. Screened from enfilade and ricochet fire by the nature and length of their works, and by the difficulty of placing the guns of the allies in favourable positions, the enemy could only be assailed by direct or vertical fire; and the troops rushing to the assault would have to advance to the attack over ground more or less open and unprotected, after leaving the shelter of their trenches.

[E. J.]

WEEKLY EVENING MEETING,

Friday, February 23.

The Rev. John Barlow, M.A. F.R.S. Vice-President and Secretary, in the Chair.

John Dickinson, Esq. F.R.S. F.G.S. M.R.I.

On providing an Additional Supply of Pure Water for London.

Mr. Dickinson commenced by describing the two different modes of supply of water to towns, namely, the one by forcing it, by means of pumping engines, directly into the pipes of supply called mains; the other by the delivery of it from a lake or reservoir on a high level, also through pipes; in which latter case the water, by the mere force of gravity, will flow over large districts, and by the comparative difference of level will rise to the tops of houses below it, to which it is conveyed by the service pipes.

He observed, that the supply by the New River Company comprehended both those modes. The river flowed into a reservoir at Islington, called the New River Head, and, of course, mains, deriving their supply from that, conveyed water to a large district of London situated below it; but, furthermore, there were pumping engines at that spot, which forced up water to reservoirs at Highgate Hill and other places, from which it was supplied to districts situated above the level of the New River Head.

He observed, that in speaking of a natural supply of water collected into a reservoir, at a high level, and delivered therefrom without the aid of pumping engines, the technical expression of engineers now is, "a supply by gravitation;" and this is the mode adopted wherever opportunity offers; which is more rare in

England than in Scotland, because in the latter country, lakes, mountains producing rivers with sharp declivities, and tracts of moor land, adapted for gathering grounds, are to be found in the neighbourhood of their principal towns and cities, and the inhabitants have availed themselves of these local advantages for obtaining the supply needed; and though, in some instances, they have had to convey water from considerable distances, and to form engineering works of great magnitude for receiving the water, yet they have to boast not only of the amplitude and excellence of their supply, but of its moderate cost.

Mr. Thom, who gave evidence before the Commission, states, " At Campbeltown, a family of five individuals will be supplied for " about 1*s.* 4*d.* per annum; the cost at Ayr, for the same quantity, " is 2*s.* 2*d.*; at Paisley, it is 2*s.* 9*d.*; in Greenock, I think, it is " about 2*s.* 6*d.* I allow in this case 5 per cent. on the capital " employed; the expense for wear and tear, charge for superintend- " ence, and the like, being always included in my estimate." He further states, " All those are high-pressure services, and reaching " the tops of houses, have all the advantages of being enabled to " put out fire, and supply the cisterns at the tops of houses."

Mr. Dickinson remarked, that the supply by gravitation had the advantage of being constant instead of being intermittent; and at the same time the works connected with it were more simple, and, by reason of saving the expense of steam engines, force-pipes, and coals, far less costly. He then proceeded to explain the mode by which it might be introduced, to a far greater extent than at present, for the supply of London, Westminster, and the western suburbs of the metropolis. The description of this was illustrated by maps, on a large scale, and a model, by means of which, and particularly by the model, the superficies of the country, and its geological features were exemplified, so as to render the description of the plan perfectly comprehensible. He explained that the model exhibited the valley of the river Lea, from whence the New River is derived, and that of the Colne, from which he proposed to derive another New River. He pointed out the uniformity of character of the rivers Lea and Colne; that each of them was constituted by the confluence of several small perennial streams issuing from the deeper valleys of the chalk, which had their source towards the summit or escarpment of that stratum, and were fed and augmented throughout every yard of their course by springs.

He explained, that in consequence of the absorbency of the surface, a considerable portion of the rainfall gradually descending through the crevices and fissures of the chalk, constitutes by its accumulation in those hills a vast natural reservoir, which, owing to the necessary difficulty of percolation towards the springs, maintains a constant supply to the rivers throughout the year, though varying exceedingly in quantity according to the amount and period of the rainfall; the summer rains being proved, according to a

system of experiment devised by Dr. Dalton,* to contribute almost nothing to the supply of this subterranean reservoir, in consequence of the great evaporation and the prodigious demands of vegetation during that period.† The speaker pointed out that either of these rivers, the Lea or the Colne, might be regarded as the outflow or yield of a large space of gathering ground, not less than 200 square miles; and the Colne had this advantage over the Lea, that the valley through which it flowed to the Thames was in perpendicular elevation much superior to that of the Lea.

Mr. Dickinson then pointed out that it was only by very long and very definite experience in the actual measurement of the river, that any one could be convinced of the enormous fluctuation in the quantity of its flow, not only according to the season of the year, but between one year and another; so that the late Mr. Telford, owing to the want of that experience, and to the neglect of seeking information from the best sources, had been led in his survey in 1834 to assume, that he could calculate upon a supply of 32 cubic feet per second from one, and that not the most considerable of the branches of the river Colne above referred to, which, at the present time, owing to the drought of last summer and autumn, does not yield much above one-third of that quantity. Mr. Dickinson gave it as his opinion, founded on more than forty years' experience, that by taking advantage of the whole supply of the valley, comprehending the four streams which are united at Rickmansworth, viz.:—the Colne, the Ver (which was the choice of Mr. Telford), the Gade, and the Chess, which latter stream flows past Latimer and Chenies, a supply of 42 cubic feet per second could always be relied upon for London, besides leaving a surplus for the lower part of the valley, which would be augmented by the stream from Missenden, which joins the Colne below Harefield;‡ and accordingly, he proposed to abstract 42 cubic feet per second at that point, for conveyance to London by a new aqueduct, constructed on more judicious principles than the New River of the Lea valley.

A plan of this work, on a very large scale, was exhibited, and the speaker explained various novel contrivances by which he proposed to give space for the deposit of every thing of greater specific gravity than water, and to intercept every thing that would float, and to clear away scum, and guard the channel from leaves and vegetable refuse, also to aerate the water; and, finally, to deliver twenty-three millions of gallons per diem—in other words, twenty-three gallons a piece daily, for a million of individuals—into a reservoir at Kilburn, so filtered, and at the same time so fresh, as to

* See article "Evaporation," by Dr. Dalton, in "Rees's Cyclopedia."

† See a very ingenious course of experiments, published by Mr. Lawes, which shows that every plant of wheat, barley, and beans, takes up in the period of its growth 15lbs. of water from the soil, if the season will afford it.

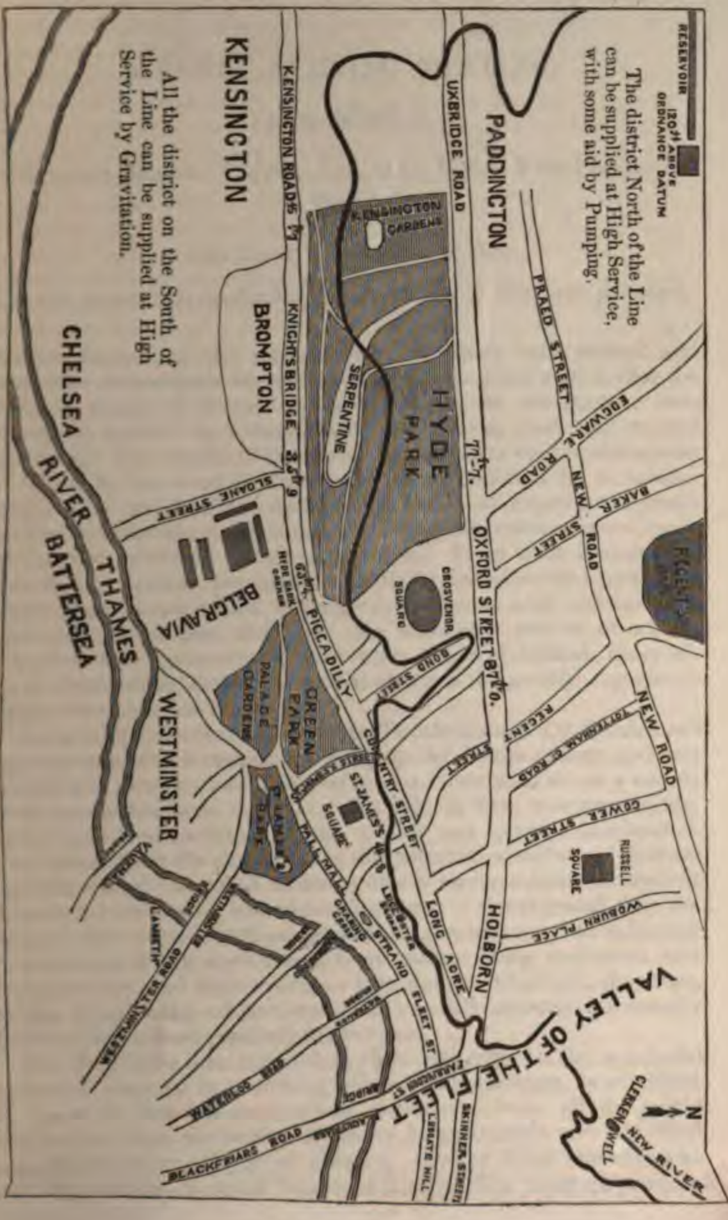
‡ The flow of water at that part, in a full season, independent of floods, is not less than four times that amount.

compare with the most perfect spring water, which reservoir, or head of distribution, would be 120 feet perpendicular above the datum line of the Ordnance Survey, made at the suggestion of the Board of Health.* The Ordnance map of London, the fruit of that survey, on the scale of 12 inches to the mile, with the elevation of every part of the metropolis stated upon it, was exhibited, which embraced also the reservoir; and, by means of a line of uniform level traced upon the map, it was made apparent how very large a portion of London could be supplied by gravitation, at high-service level, over and above the whole of Westminster, Belgravia, Knightsbridge, Brompton, Chelsea, Fulham, and Kensington. Mr. Dickinson, having had a great deal of experience in works of this nature, was satisfied that the cost of delivering this quantity of water at Kilburn, purified and filtered, (presuming money to be obtainable at 4 per cent. per annum,) would not (according to Mr. Thom's mode of estimate) exceed three farthings per thousand gallons, including the very heavy item of compensation to mill-owners; and that the whole cost of distribution would not exceed threepence per thousand gallons, which, as proved in the evidence of Mr. Hawksley, was the rate of charge for the water supply at Nottingham.

Some of the water taken out of the river Colne, at Harefield, was produced at the meeting and much approved. With reference to the liability of the Colne being rendered turbid at the time of floods, (for at other times it is perfectly clear,) he quoted the following evidence of Mr. Hawksley on the subject of running water purifying itself,—"I can give a very extraordinary instance of "that, as occurring at Nottingham. At Nottingham the supply is "taken from the river Trent. Upon the tributaries of the river "Trent are situated the towns of Leicester, Loughborough, Derby, "Belton, and the whole of the Potteries. The water leaves those "towns frequently in an exceedingly black noisome state, but the "water of the river Trent is, nevertheless, exceedingly beautiful "and pellucid; in fact, at Trent Bridge, near Nottingham, it is as "clear as crystal; organic matter is not discoverable in it, except "in the degree in which it is discoverable in all river water."

The speaker concluded by stating that he had taken particular notice of the amount of water expended in his own house and stables, (in Upper Brook Street,) and it led him to the conviction that the whole of the large and populous district, before referred to, southward of, and including Piccadilly, might be supplied by this application of the gravitation system at one-fourth the scale of the present rate of charge; but with the proviso, that water for the streets and for public purposes, should be paid for out of the parish rates as at present. [J. D.]

* This quantity is almost exactly double that of the New River, the total supply of which is stated by Mr. Mylne, the engineer of that Company, in his evidence before the Parliamentary Committee of Inquiry, respecting the water-supply of London, Qu: 3073, to be 11,872,000 gallons per diem.



MAP, showing the Application of this Water Supply to the Metropolis; the figures denote the height of the respective Localities above the Datum Line of the Ordnance Survey.

WEEKLY EVENING MEETING,

Friday, March 2.

WILLIAM ROBERT GROVE, Esq. Q.C. F.R.S. Vice-President, in the Chair.

DR. JOHN STENHOUSE, F.R.S.

On the economical applications of Charcoal to Sanitary purposes.

AFTER describing the various ways in which both animal and vegetable charcoal are manufactured, Dr. Stenhouse stated, that the different kinds of charcoal most commonly in use may be conveniently divided into three species, viz. wood, peat, and animal charcoal. The results of Saussure's experiments on the absorption of gases by boxwood charcoal were then exhibited in a tabular form. The speaker then described a series of experiments made by him to ascertain the comparative absorbent power of wood, peat, and animal charcoal for gaseous bodies. From these it appeared, that wood charcoal possesses a slightly higher absorbent power for ammoniacal, sulphuretted hydrogen, sulphurous acid, and carbonic acid gases than peat charcoal; the absorbent powers of which, however, are immensely greater than those of animal charcoal. As a decolorizer, however, animal charcoal is greatly superior to either wood or peat charcoal.

An account was next given of Mr. Turnbull's and Dr. Stenhouse's experiments, which consisted in burying the bodies of dogs and cats in charcoal powder, and in covering them over with about a couple of inches of the same material. No effluvia were ever perceptible, while the decomposition of the bodies was greatly accelerated. This arises from the circumstance that charcoal absorbs and oxidises the effluvia, which would under ordinary circumstances be evolved directly into the air; but within the pores of the charcoal they are brought into contact with condensed oxygen, and are thus subjected to a species of low combustion, their carbon being converted into carbonic acid, and their hydrogen into water. Charcoal, therefore, so far from being an antiseptic, as was till recently universally believed, is, in fact, precisely the reverse.

Dr. Stenhouse then stated that, from reflecting on the wonderful power of charcoal in absorbing effluvia and miasmata, as exhibited in the cases just described, where, as we have seen, all the putrid exhalations from the bodies of pretty large animals were absorbed and destroyed by a layer of charcoal powder little more than an inch in thickness, it struck him that a very thin layer of powdered

charcoal would be equally effectual in absorbing the very minute quantity of infectious matter floating in the atmosphere of what are called unhealthy situations. This led him to the construction of the so-called Charcoal Air-filter, first exhibited and described by him before the Society of Arts, on the 22nd of February, 1854. It consists of a thin layer of charcoal powder, enclosed between two sheets of wire gauze. One of these air-filters, or charcoal ventilators, was erected more than three months ago in the justice-room, at the Mansion-house. This apartment, from the position of several nuisances in the very narrow street from which it is ventilated, was usually so offensive as to have become the subject of general complaint. Since the erection of the charcoal ventilator, through which all the air entering the apartment is made to pass, all the impurities are absorbed, and the atmosphere of the room has become unexceptionable. From the success attending on the charcoal ventilator at the Mansion-house, the City authorities have fitted up the justice-room at Guildhall with a similar apparatus, which is giving equal satisfaction. The charcoal ventilator at the Mansion-house has never required any alteration, such as renewal of the charcoal, or otherwise. Charcoal ventilators cannot fail to prove eminently useful in all situations where foul air is apt to accumulate, such as in water-closets, in the close wards of hospitals, in ships, and in the back courts and mews-lanes of large cities, all the impurities being absorbed and retained by the charcoal, while a current of pure air alone is admitted into the neighbouring apartments. In this way pure air is obtained from exceedingly impure sources.

A short sketch was then given of the history and construction of Respirators, from their first proposal by Dr. Beddoes of Bristol, in 1802, till their description, some seventeen or eighteen years ago, by Dr. Arnott, in a lecture at the Royal Institution, and their being subsequently patented by Mr. Jeffreys, who first brought them into general use. Mr. Jeffreys' and the ordinary respirators are intended merely to warm the air; but the charcoal respirators, especially those which embrace both the nostrils and mouth, purify the air by filtration, and thereby deprive it of the noxious miasmata which, in unhealthy situations, it not unfrequently contains. Experience has shown, however, that charcoal respirators not only purify the air, but warm it sufficiently, while they possess several advantages over the ordinary respirators. Thus, for instance, they are lighter and more easy of construction; and where the breath is at all fœtid, as is usually the case in diseases of the chest, throat, &c. the disagreeable effluvia are absorbed by the charcoal, so that pure air alone is inspired. The charcoal respirators are also exceedingly easy to breathe through, as, owing to the non-conducting nature of their material, they do not condense the moisture of the breath to an inconvenient extent. There are three forms of the charcoal respirator, one for the mouth alone, the others embracing both the mouth and nostrils; these two latter forms being specially intended

to guard the wearer against fevers, and other infectious diseases. Powdered charcoal has, during the last twelve months, been most successfully employed both at St. Mary's and St. Bartholomew's Hospitals, and in other similar establishments, to arrest the progress of gangrene and other putrid sores. In the case of hospital gangrene we have to deal not only with *effluvia* but with real *miasmata;* for gangrenous sores not only affect the individual with whom the mischief has originated, but readily infect the healthy wounds of any persons in its vicinity. In this way gangrene has been known to spread not only through one ward, but through all the wards of even a large hospital. This, and other instances which might easily be adduced, prove that charcoal is not only a deodorizer but a very efficient disinfectant. A great variety of other instances were mentioned, in which charcoal respirators would certainly prove exceedingly useful; such, for instance, as to house-painters, the gunners in casemated batteries, persons requiring to traverse unhealthy districts within the tropics, such as the Delta of the Niger, the foot of the Himalaya, &c.

Dr. Stenhouse concluded by stating it as his confident belief, that if our soldiers and sailors, when placed in unhealthy situations, were furnished with charcoal respirators, and if the floors of their tents, and the lower decks of ships were covered by a thin layer of freshly-burned wood charcoal, we would have little in future to apprehend from the ravages of cholera, yellow fever, and similar diseases. [J. S.]

GENERAL MONTHLY MEETING,

Monday, March 5.

Frederick Pollock, Esq. M.A. in the Chair.

J. Richard Andrews, Esq.
John Baily, Esq. Q.C.
Charles Beevor, Esq. F.R.C.S.
Henry Bradbury, Esq.
Henry Newnham Davis, Esq.
J. Dickinson, Esq. F.R.S., G.S.
John Viret Gooch, Esq. F.S.A.
Rev. Geo. Delgarno Hill, M.A.
Edward James, Esq. M.A.Q.C.
Robert Lee, M.D. F.R.S.
Walter M'Grigor, Esq.
Leopold Redpath, Esq.

were duly *elected* Members of the Royal Institution.

George J. Lyons, Esq.
Edmund Macrory, Esq. and
John William Wrey, Esq.

were *admitted* Members of the Royal Institution.

The Secretary reported that the following Arrangements had been made for the Lectures after Easter :—

Eight Lectures on VOLTAIC ELECTRICITY, by PROFESSOR TYNDALL, F.R.S.

Eight Lectures (with Illustrations) on CHRISTIAN ART, FROM THE EARLIEST PERIOD TO RAPHAEL AND MICHAEL ANGELO, by GEORGE SCHARF, Jun. Esq. F.S.A. F.R.S.L.

Eight Lectures on ELECTRO-PHYSIOLOGY, by DR. DU BOIS-REYMOND.

The following PRESENTS were announced, and the thanks of the Members returned for the same :—

FROM

Agricultural Society of England, Royal—Journal. Vol. XV. Part 2. 8vo. 1855.
Asiatic Society of Bengal—Journal, No. 244. 8vo. 1854.
Astronomical Society, Royal—Monthly Notices. Vol. XV. No. 3. 8vo. 1855.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for February, 1855. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for February, 1855. 4to.
Bradbury, Henry, Esq. (the Author)—A Few Leaves illustrative of Nature-Printing. fol. 1854.
Botfield, Beriah, Esq. M.R.I. (the Author)—Notes on Libraries. 8vo. 1855.
British Architects, Royal Institute of—Proceedings in February, 1855. 4to.
Civil Engineers, Institution of—Proceedings in February, 1855. 8vo.
Editors—The Medical Circular for February, 1855. 8vo.
The Athenæum for February, 1855. 4to.
The Practical Mechanic's Journal for March, 1855. 4to.
The Mechanics' Magazine for February, 1855. 8vo.
The Journal of Gas-Lighting for February, 1855. 4to.
Deutsches Athenäum for February, 1855. 4to.
Faraday, Professor, D.C.L. F.R.S. (the Author)—On some Points of Magnetic Philosophy. (From Phil. Mag. Feb. 1855.) 8vo.
Monatsbericht der Königl. Preuss. Akademie, December 1854. 8vo.
Franklin Institute of Pennsylvania—Journal, Vol. XXIX. No. 1. 8vo. 1854.
Geological Society—Quarterly Journal, No. 41. 8vo. 1855.
Graham, George, Esq. Registrar-General—Report of the Registrar-General for February, 1855. 8vo.
Lindsay, W. L. M.D. (the Author)—Experiments on the Dyeing Properties of Lichens. 8vo. 1854.
Lovell, E. B. Esq. M.R.I. (the Editor)—The Monthly Digest for December 1854, and January and February, 1855, and Annual Volume for 1854. 8vo.
Macrory, E. Esq. M.R.I. (the Author)—Reports of Cases relating to Letters Patent for Inventions. Part 1. 8vo. 1855.
North of England Institute of Mining Engineers—Transactions, Vol. II. 8vo. 1853–4.
Novello, Mr. (the Publisher)—The Musical Times for February, 1855. 4to.
Photographic Society—Journal, No. 27. 8vo. 1855.
Royal Society of London—Philosophical Transactions, 1854. Vol. CXLIV. Part 2. 1854.
Proceedings, Vol. VII. No. 8. 8vo. 1855.
List of Members, 1854. 4to.
Society of Arts—Journal for February, 1855. 8vo.
Statistical Society of London—Journal, Vol. XVIII. Part 1. 8vo. 1855.
Taylor, Rev. W. F.R.S. M.R.I.—Magazine for the Blind, February, 1855. 4to.
Vereins zur Beförderung des Gewerbfleisses in Preussen.—Verhandlungen, November und December, 1854. 4to.

WEEKLY EVENING MEETING,

Friday, March 9.

Rev. John Barlow, F.R.S. Vice-President and Secretary, in the Chair.

Thomas Sopwith, F.R.S. F.G.S. M.R.I.
MEMBER OF THE GEOLOGICAL SOCIETY OF FRANCE.

On the Mining Districts of the North of England.

Mr. Sopwith described the North of England as the central portion of the island of Great Britain, lying midway between the extreme north of Scotland and the south coast of England; well defined in three directions, by the Scottish border on the north, and by the ocean on the east and west: midway also, as regards its surface elevations, between the level of the sea and the highest mountains of Britain; and the chief strata of this district as also nearly midway in the well-defined series of stratified rocks. Southward, its limits are less definite; but the most important mines of coal, iron, and lead, being in the counties of Northumberland, Durham, and Cumberland, the illustrations were at present confined to these counties. The importance of these minerals imparts interest to the several circumstances connected with them; and the models, maps, and drawings exhibited were selected in order to present, as clearly as could be done in the brief limits of an hour's discourse, the conditions of physical geography, the detailed as well as the general position, depth, and sub-divisions of strata, the mode of working coal, iron, and lead, and the character of the more remarkable antiquities.

A large map, on a scale of one inch to a mile, showed, by appropriate colouring, the extent of country drained by the rivers Coquet, Wansbeck, Blyth, Tyne, Wear, and Tees, on the eastern side of the district, and by the river Eden on the western side towards Carlisle. Of these rivers, the Tyne is at once seen to be the most extensive and important. The hills between these rivers, and the lands adjoining them, seldom exceed 2000 feet in height, and it is only recently that any correct measurement of the elevations has been commenced by the Ordnance Survey. Some of these were described as indicating the height of the moors, where traversed by the turnpike road from Middleton in Teesdale, to Alston—the 10th milestone, southward from Alston, being 1880·39 ft. above the mean level of the sea, and the 8th milestone on same road, 1963·37 feet. The church

at Alston, at 3·62 feet above the surface, is 956·80 feet above sea level. The highest summit of this range of mountains is, at Crossfell, about 2900 feet. The detailed section of strata and mines at Allenheads represents a tract of country varying from 1400 feet to 2200 feet. The meteorological conditions of different parts of the district consequently vary considerably; and a diagram, prepared by Mr. Bewick, illustrated the exact range of observations of barometer, thermometers, and rain guage, during the months of December 1854 and January 1855. The position of the strata, as regards facilities for mining, is mainly dependent on the elevation of the country. This was illustrated by a model, made in separate moveable parts, showing the coal measures, millstone grit, and mountain limestone formations, and representing, first, the condition of the several rocks previous to being dislocated by a fault; secondly, the effects of such a fault, or break of the strata, by which on one side the strata are depressed; and thirdly, the subsequent results of denudation, or wearing away of the surface by water. The direction of the great Tynedale fault was shown on the large map, and the amount of depression varies from 500 to 1000 feet.

The situation of the principal coal-fields of the North of England was also described, as also the several harbours, of the number and extensive improvements of which various details were given, and some of the chief circumstances connected with the working of coal mines, of which several diagrams were exhibited. From the river Coquet to the Tees, the coal-fields of Northumberland and Durham extend along the coast a distance of about 50 miles: the extreme breadth is nearly 25 miles, and the average breadth about 16 miles; the total area from 700 to 800 miles. The entire series of coal seams or beds, by no means extend over so large a space, and different qualities of coal abound in separate portions of the district.

The application of cages, introduced by Mr. T. Y. Hall, effected great economy in the methods of bringing coal to the surface. The several coal seams, and the accompanying beds of silicious and other strata, as found at Townley colliery, were shown by a large section and drawing which had been expressly prepared for the purpose of illustrating, on a large scale, the general outline of colliery workings, by Mr. J. B. Simpson, son of the viewer of that colliery, and it furnished, at the same time, an excellent specimen of delineation of such objects.

Open spaces in the midst of the plan showed what is locally termed the *broken* or *waste*, being the excavated coal wholly removed; but the greater part of the diagram was occupied with a representation of the preliminary operations which, in the first instance, constitute the ordinary workings of a coal mine or colliery.

Access to the coal is obtained by means of vertical shafts. One of these is the *downcast shaft*, the other the *upcast shaft*, which names are derived from the important use of these shafts in ven-

tilating the mine. The air descends, or goes *down* the downcast shaft, and then, after having traversed the workings of the colliery, rises *up* the upcast shaft. The depth of the downcast shaft represented was about 68 fathoms, *i.e.* 408 feet, somewhat more than the height of St. Paul's dome, and almost exactly double the height of the Monument, which is 202 feet. Pillars of coal, varying from 40 to 50, 80, or 100 yards, are left to support the strata round the shaft —in this instance they are 50 yards square; the diameter of the shaft is 13 feet. The winning head-ways, boards, pillars, &c., were described by reference to the plan.

After adverting to the localities which formerly produced or do now produce household, coking, or steam coal, allusion was made to the Roman wall, the east termination of which, midway between Newcastle and the sea, gave rise to the well-known name of *Wallsend* coal—a large colliery at that place, (which was also the residence of the eminent viewer, Mr. Buddle,) having produced an excellent description of household coal; and the high estimation of *Wallsend* coal led to the appellation being extended to others. At present vast quantities of the best household coal are produced from collieries in the districts near Haswell, Hetton, Seaham, &c., south of the river Wear. Coking coal abounds in the western part of the coal-fields; and steam coal is extensively worked in several parts of the county of Northumberland, chiefly in an area of fifty square miles, between the rivers Coquet and Tyne—the locality of the principal collieries being indicated by railways, or *waggon ways.* These various qualities of coal, owing to the vast development of steam navigation, railway locomotion, the iron trade, and various chemical and manufacturing processes, in the last thirty years, have attained an importance far exceeding that which once appertained to the *Wallsend* coal of the north banks of the Tyne.—The Roman wall itself received a passing notice, as one of the most remarkable antiquities of the North of England;* and drawings were exhibited of some of its more conspicuous features. The connexion between the conditions of physical geography and the works of human art was exemplified in the fact of the Romans having diverged from a direct line, to avail themselves of steep and romantic precipices formed of basalt, the overflowing of which, in the midst of regularly stratified rocks, is especially deserving of note, both as a geological and mining condition, and as an index to several of the most remarkable objects, both of nature and art, in the mining districts of the North of England. The greatest known mass of this basalt is found near the rise of the river Tees; and Mr. Burlison, of Durham, has recently made a careful painting, which

* The excellent work of Dr. Bruce, of Newcastle, on the Roman Wall, has already passed through two editions, and a third is in preparation. The diagrams of the wall exhibited were from the numerous and graphic illustrations of Dr. Bruce's work.

was placed on the table, showing the basaltic rocks at the waterfall of Cauldron Snout. Passing northward, and somewhat west of the limits of the Northumberland coal-fields, the basalt is found near Alnwick, in the pleasure-grounds of the Duke of Northumberland, at Dunstanbrough Castle, Bamburgh Castle, Holy Island Castle, and at Farn Islands.

The production of the coal mines of the Northumberland and Durham district now reaches an amount little, if any, short of fourteen millions tons annually. In round numbers, and as conveying a general approximation, it may be considered that of this quantity six millions are destined for London and the coast trade, and about two and a half millions exported abroad; the consumption of coal for coke (inland, coast, and foreign) is about two and a half millions; colliery engines and workmen consume upwards of a million tons; and the ordinary local consumption of the district may be taken at about two millions. Of this enormous quantity, a conception can only be formed by reducing it to some other standards of comparison, as for example:—This quantity of coal, if formed into blocks of one cubic yard each, would cover about four square miles; and if the same quantity of coal be considered as forming the coating of a road, one inch thick and six yards wide, it would extend considerably more than four thousand miles. Blocks of one cubic foot can be readily comprehended; and if one person were employed to count these blocks at the rate of three thousand six hundred in every hour, and thirty-six thousand every day, it would occupy him more than ten years to complete his task.

The variable thickness of different coal-seams was adverted to, and the number and thickness of the seams or beds of coal in the North of England described. Several illustrations were shown from an able work recently completed by Mr. Greenwell on Mine Engineering. The thickness of the Newcastle seams varies from an inch to five and a half feet; the aggregate thickness of nearly sixty seams amounts to about seventy-five feet, or nearly four per cent. of the entire mass of strata. Nine only of these beds exceed two and a half feet, and the aggregate quantity of workable coal is, therefore, only about one-half of the above quantity. The depth at which the mines are worked was shown by several examples, varying from nearly three hundred fathoms (eighteen hundred feet) at Monk-Wearmouth, to shallow pits, worked at a small depth from the surface, near the outcrop of the coal. The detailed maps, by Mr. J. W. Bell, of Newcastle, one of which was shown, exhibit the boundaries of property in the coal field, and form a valuable local record of the position and extent of the various collieries.

The general situation and extent of the *Lead* mining district was described by reference to the map, and a number of plans and sections explained the manner in which mineral veins occur, and the details of works by which access is had to them. Accurate returns of the produce of lead and other minerals are now

obtained for Government, by the instrumentality of the Mining Record Office, established in connection with the Museum of Economic Geology.* The total produce of lead has been estimated at about one hundred thousand tons; and, as a rough approximation of the last few years, it may be considered that six-tenths of this, or sixty thousand tons, are raised in Great Britain—England alone producing about forty thousand tons. The North of England lead mining districts furnish about twenty thousand tons, and one moiety of this is raised in the W.B. mines of Mr. Beaumont—the initials W.B. (William Blackett) being the well known trade mark of the lead produced from these, the most extensive lead mines in the world. The annual produce, when formed into one and half-stone *pigs* or ingots of lead would, if laid in a direct line, extend about seventy miles. In these mines water-pressure engines were first introduced about eighty years ago; and within the last five years a still more important application of water power has been made by the use of the hydraulic engines patented by W. G. Armstrong and Co., of Newcastle.

The existence of vast deposits of *Iron* ore near the mouth of the Tees and in various other localities, as also the rapidly increasing development and importance of the iron trade in the North of England, were briefly adverted to. Little more than one hundred years ago the quantity of iron made in this kingdom was about twenty-five thousand tons, and at the beginning of this century one hundred and seventy thousand tons. Fifteen years ago this quantity had increased to one and a half millions of tons, and at present the production reaches, and probably exceeds, two and a half millions of tons.

A description of several architectural antiquities was given with reference to drawings, prepared under the direction of Dr. Bruce, of Newcastle; amongst which were portions of Norham Castle, on the Tweed, and Richmond Castle, in Yorkshire, exhibiting the massive character of these strongholds; the entrance gateway of Alnwick Castle, and Norman doorways at the Castle in Newcastle, and in Durham Cathedral and Castle. Some of these examples are remarkable for richness of architectural detail, others for a simplicity of style, which affords a useful model—combining economy with the appropriate expression of the Norman character of building.

The principal towns in this part of the North of England were shown on the large map by circles of red colour, varying according to the amount of population. Of this only a rapid notice could be given in round numbers: Newcastle, the chief town, contains about

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* So called at its first establishment, a name which admirably denotes its objects and utility; subsequently named "Museum of *Practical* Geology," and still more recently amalgamated with more extended objects, under the Art and Science Department of the Board of Trade.

ten thousand houses, and eighty-eight thousand inhabitants. Gateshead, on the opposite bank of the Tyne, has three thousand five hundred houses, and twenty-five thousand inhabitants—together, a population of one hundred and thirteen thousand. North and South Shields, lying on opposite sides of the Tyne at its mouth, have nearly eight thousand houses, and sixty thousand inhabitants; and this amount is somewhat exceeded by Sunderland. These towns, with the city of Durham, altogether contain upwards of thirty thousand houses, and more than a quarter of a million inhabitants, being about one-tenth of the population and houses of the Metropolis; the proportional number of inhabitants to a house is nearly the same. The number of persons to a square mile in Northumberland is one hundred and fifty-four; in Cumberland, one hundred and twenty-five; but in Durham, owing to the great number and extent of colliery and manufacturing operations, the population is four hundred to the square mile; and from the same influence of mining conditions, the increase of population in fifty years, from 1801 to 1851, which in Northumberland has been 79 per cent., and in Cumberland 66 per cent., has been in Durham 160 per cent.

Several illustrations of the geology and mining of the North of England, were separately noticed, as conveying a clearer idea than could be conveyed by mere verbal description; amongst these were sections of strata in various parts of the lead mine districts, showing the remarkable conformity which prevails over about 400 square miles of the country near the junction of the several counties of Northumberland, Durham, Cumberland, Westmoreland, and Yorkshire, also sections of lead mines, showing the mode of access by horizontal adits or levels, and vertical shafts, with the several galleries and other excavations, for obtaining lead ore from veins: the position of these relatively to the strata was delineated on several drawings.

[After the lecture, several excellent microscopic sections of coal, from the North of England, were shown by Mr. Sopwith, in the Library.]

[T. S.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, March 16, 1855.

SIR HENRY HOLLAND, Bart. F.R.S. Vice-President, in the Chair.

DR. WM. ODLING, F.C.S.

On the Constitution of the Hydro-carbons.

EVERY chemical compound may be regarded in a great number of different aspects. Each of the different theories that have been propounded concerning the chemical constitution of bodies, is true in reference to one particular aspect,—untrue in reference to all others. Theories are of the highest service when they enable us to look upon a larger number of bodies from a single point of view,—of the highest detriment, when they prevent us from making use of all other points of view. To regard a body, or a class of bodies, exclusively in one aspect, or, in other words, to view all compounds by the light of a single theory, is necessarily to neglect a whole host of phenomena and relations. He has the most complete knowledge of a compound, who is capable of changing his position, and looking at the body from every possible point of view.

The theory of compound radicals is of the utmost service in enabling us to look upon a large class of bodies in one single aspect, in affording us one of the best means of arrangement, comparison, and explanation: but it has no pretensions whatever to represent the entire and absolute truth with regard to the constitution of bodies; it simply exhibits them from one of many excellent points of view; it has reference less to the actual constitution of the bodies, than to our particular mode of regarding them.

In proportion to the complexity of the constitution of a body, so is the number of aspects in which it may be regarded, so is the number of rational theories that may be entertained concerning it. All of these theories belong to the same order of truth: they differ from one another only in their greater or less degree of generality. The theory of the greatest generality most nearly approximates to the representation of bodies, especially typical bodies, by empirical formulæ, as unitary molecules.

Adopting the proportional numbers of Gerhardt, we represent the two-volume molecules of muriatic acid, water, ammonia, and coal-gas, by ClH, OH^2, NH^3, CH^4 respectively. In accordance

with certain theoretical notions, these bodies have been formulated as follows:—

H·Cl			
H·OH (*Laurent.*)	H^2·O		
H·NH^2 (*Kane.*)	H^2·NH (*Wurtz.*)	H^3·N	
H·CH^3 (*Liebig.*)	H^2·CH^2 (*Dumas.*)	H^3·CH (*Odling.*)	H^4·C

Coal-gas may be represented as terhydride of formyl, analogous to its derivative chloroform, or terchloride of formyl. The two bodies can be prepared in virtue of analogous equations from acetic and chlor-acetic acids respectively, and the one can be obtained from the other by direct substitution.

Each of the above theories has certain circumstances in its favour; each is true to a certain extent; each represents the body in question from a different point of view; sometimes one point of view is most advantageous, sometimes another. As a veritable representation of the constitution of coal-gas, Dumas' view is preferable to either of the other two theoretical views.

The adoption of Laurent's sarcastic suggestion of peroxide of hydrogen as a compound radical, leads to inadmissible or uncertain results; thus, is potash oxide of zinc K Z O a combination of a hypothetical peroxide of potassium with zinc, or of a hypothetical peroxide of zinc with potassium? Is Williamson's double ether, Me Et O, a combination of peroxide of methyl with ethyl, or of peroxide of ethyl with methyl? &c.

Nevertheless, there are greater grounds for recognising peroxide of hydrogen as a compound radical, than there are for recognising ethyl and methyl as such. A large number of bodies may be represented very feasibly as containing ethyl; but an infinitely larger and more varied set of bodies may be represented as containing peroxide of hydrogen: such, for instance, are water, potash, sulphuric acid, formic acid, benzoic acid, hypochlorous acid, &c. &c. and, as has been shown by Mr. Brodie, very many other more complicated bodies. Many equations may be represented very simply by means of ethyl analogous to hydrogen; but a much greater number may be represented by means of peroxide of hydrogen analogous to chlorine. Ethyl has been obtained in the free state, so has peroxide of hydrogen; but whereas nearly all the bodies of the peroxide of hydrogen series can be obtained directly from *it*, not one single ethylic combination has ever yet been obtained from ethyl. Hydrogen and ethyl present certain analogies, but the analogies of chlorine and peroxide of hydrogen are much more complete. Both bodies bleach, oxidise, combine directly with potassium, set free bromine and iodine, and take the place of the bromine and iodine set free. In Ba·OH and in Ba Cl the Ba

can be readily detected; but with regard to H Cl and Et Cl, the Cl can be detected in the former only.

All the facts connected with the mutual relations of

$C^2 H^4$	—ethylene, or olefiant gas
$C^2 H^4 O$	—aldehyd
$C^2 H^4 O^2$	—acetic acid
$C^2 H^6$	—hydro-ethylene
$C^2 H^5 Cl$	—muriatic ether
$C^2 H^6 O$	—alcohol,

especially since the recent researches of Berthelot, show the superiority of Dumas' ethylene to Liebig's ethyl theory, both as regards its more complete accordance with experiment, and its greater generality. The probabilities in favour of the pre-existence of $C^2 H^4$ and its derivatives, as constituent groups, are much greater than are those in favour of the pre-existence of $C^2 H^5$. Thus, with regard to ethylene, hydro-ethylene, muriatic ether, and their chlorine derivatives, we ought to have the following series, convertible into one another through certain members:—

			$C^2 H^4 \cdot H^2$
		$C^2 H^4 \cdot H Cl$	$C^2 H^3 Cl \cdot H^2$
$C^2 H^4$	$C^2 H^4 \cdot Cl^2$	$C^2 H^3 Cl \cdot H Cl$	$C^2 H^2 Cl^2 \cdot H^2$
$C^2 H^3 Cl$	$C^2 H^3 Cl \cdot Cl^2$	$C^2 H^2 Cl^2 \cdot H Cl$	$C^2 H Cl^3 \cdot H^2$
$C^2 H^2 Cl^2$	$C^2 H^2 Cl^2 \cdot Cl^2$	$C^2 H Cl^3 \cdot H Cl$	$C^2 Cl^4 \cdot H^2$
$C^2 H Cl^3$	$C^2 H Cl^3 \cdot Cl^2$	$C^2 Cl^4 \cdot H Cl$	
$C^2 Cl^4$	$C^2 Cl^4 \cdot Cl^2$		

Of these four series, three are undoubtedly, and the fourth most probably, known to us. They illustrate rationally the nature of the isomerism. In the three latter series, we have every reason to believe, that, with regard to the carbon, two of the hydrogen or chlorine atoms stand in a different relation to the other four; but in the first series, we have not a single fact tending to show, that one of the hydrogen atoms stands in a different relation to the other three; not one fact to countenance the representation of olefiant-gas by $C^2 H^3 \cdot H$, hydruret of acetyl.

In the next best known hydro-carbon, namely, benzine, there is no more reason for believing in the existence of the monobasic radical phenyl $C^6 H^5$, than there is for believing in the bibasic and tribasic radicals $C^6 H^4$ and $C^6 H^3$, respectively, as seen in the following table:—($X = NO^2$ $Ad = NH^2$)

$C^6 H^5 \cdot H$	$C^6 H^4 \cdot H^2$	$C^6 H^3 \cdot H^3$
$C^6 H^5 \cdot Cl$	$C^6 H^4 \cdot Ad^2$	$C^6 H^3 \cdot Cl^3$
$C^6 H^5 \cdot Br$	$C^6 H^4 \cdot X^2$	$C^6 H^3 \cdot Br^3$
$C^6 H^5 \cdot Ad$	$C^6 H^4 \cdot X\ Ad$	$C^6 H^3 \cdot Ad^2 X$
$C^6 H^5 \cdot X$	$C^6 H^4 \cdot Cl\ Ad$	$C^6 H^3 \cdot X^2 Ad$
	$C^6 H^4 \cdot Br\ Ad$, &c.	$C^6 H^3 \cdot X^2 Cl$, &c.

Lastly, Williamson's othyl theory, although it possesses a great degree of generality, and is supported by most complete analogies both in mineral and in organic chemistry, is only one of many ways of indicating the mutual relations of bodies. It must not be taken as the sole veritable representation of the constitution of the compounds to which it applies. There are no greater proofs of the pre-existence of othyl in acetic acid, than there are of the pre-existence of peroxide of hydrogen in water.

For example, the correlations of benzamide, benzonitryl, hydrobenzamide, and dibenzoylimide, are entirely neglected in the othyl theory. These bodies belong to one single class; they all contain certain benzoic elements, and certain ammoniacal elements; by the absorption of water they yield ammonia, and benzoic acid or aldehyd. But the othyl theory bears no reference to this point of view; it separates benzamide widely from its congeners. Thus we are told that the first body contains the compound radicals benzoyl (analogous to othyl) and amidogen; the second, the compound radicals phenyl and cyanogen; the third, nitrogen and the compound radical benzyl (analogous to acetyl), whilst, with regard to the fourth, as to many other bodies, the compound radical theory fails altogether.

In the three best known hydro-carbons, coal-gas, olefiant-gas, and benzine, as in many other bodies ordinarily represented as containing compound radicals, the conception of self-existent constituent compound radicals, is not only unnecessary but irrational. The particular groupings of atoms, which we denominate compound radicals, do not have an existence apart from the other constituents of the bodies, into which they are said to enter.

[W. O.]

WEEKLY EVENING MEETING,

Friday, March 23.

Henry Bence Jones, M.D. F.R.S.
in the Chair.

Rev. John Eyre Ashby,

On (so-called) Catalytic action and Combustion; and theories of Catalysis.

The study of Catalysis is a study of forces. It comprehends the conditions under which force is exerted in a peculiar manner in *chemical decompositions* and combinations, and of the nature of the *force only as* compared with forces at work in chemical changes,

which are commonly thought to be better understood. The precise meaning of the word should not be derived from its etymological constitution, nor even solely from its original application, but rather from the general consent of eminent chemists as exhibited in their published writings. The term has been employed very loosely, and some of the definitions given do not allow of reduction to a common statement. Adopting the principle laid down, catalysis may be defined as the action by contact of one substance upon another substance, or group of substances, whereby chemical changes are effected, while the first substance remains finally unchanged. This will *exclude* from catalysis the following cases:—

1. The operating body does change, although it does not combine.

Examples.—Fermentation, as explained by Liebig. Solution of an alloy of platinum and silver in nitric acid; also of the alloy of copper, zinc, and nickel, in dilute sulphuric acid.

2. The operating body absorbs A from a combination (A + B), and B cannot exist free.

Example.—Crystallized oxalic acid is a combination of anhydrous oxalic acid and water; if cast into strong sulphuric acid, the water is absorbed, and (so far as we know at present) taken into chemical union with the sulphuric acid; but the anhydrous oxalic acid cannot exist free, and is resolved into equal volumes of carbonic acid and carbonic oxide, which escape accordingly.

3. The operating body takes from a substance some of its elements, and combines with them after they have combined with each other.

Example.—Sulphuric acid in contact with sugar, takes the elements of water from the carbon, and combines with them by hydration.

4. The operating body causes mutual changes in substances A and B, whereby they become other substances, say C and D, and the operating body combines with D.

Example.—Hydrocyanic acid and water + hydrochloric acid (the operating body) gives formic acid and ammonia, of which the ammonia combines with the hydrochloric acid.

$$C_2NH + 3HO \text{ becomes } NH_3 + C_2HO_3$$
$$\text{and we have } NH_4Cl + C_2HO_3.$$

Cases of chemical change by friction and percussion are excluded, because *contact* is mechanically statical, whereas these are dynamical in respect of the masses. A lucifer match is an example. Sir James Kane, however, supposes the decomposition of iodide of nitrogen by slight percussion to be catalytic; his definition is made wide enough to take it in.

The definition given by the speaker as being, upon the whole, in harmony with the prevailing ideas of catalysis, will be found to *include* the following cases:—

1. Galvanic action in relation to a finally unchanged platinode. In every battery-cell containing an apparently unchangeable platinode, all the conditions of catalysis are perfectly fulfilled when the platinode touches the zincode without the intervention of any other metallic conductor; and the truthfulness of the fulfilment is not altered by the additional fact that similar action will ensue when a metallic (or other) conductor is interposed. It is very doubtful whether the platinode really undergoes no changes, but it remains unchanged at last.

2. A finally unchanged body produces in certain others no decomposition, but only combination.

Example.—Platinum combines hydrogen and oxygen into water.

3. A finally unchanged body produces decomposition, but no re-combination.

Example.—Decomposition of chlorate of potash by metallic oxides and heat.

4. A finally unchanged body produces decomposition, and partial or total recomposition, of the elements of a body.

Example.—Alcohol passed into sulphuric acid at 300° passes out as ether and water, the elements of water having been abstracted by the acid, which cannot retain them at that temperature. If we suppose that the acid removes water, *as such*, from the alcohol, this case is resolved into the foregoing.

5. A finally unchanged body produces the decomposition of another substance, and causes some of its elements to enter into combination with a third body, also present.

Examples.—Heated platinum decomposes alcohol vapour, with access of air. In illustration of this class of phenomena, the speaker exhibited the continuous combustion of the vapour of strong liquid ammonia by a flat spiral of platinum wire ($\frac{1}{70}$ inch thick) evolving nitrous gas and water.

Mr. Ashby then briefly adverted to the principal theories of Catalysis.

1. Berzelius, who introduced the term into chemistry, considers that it represents a new force.

2. Liebig dwells at great length upon cases in which the operating body does *not* remain finally unchanged. He supposes that if A and B are in contact, and changes happen among the particles of A, these may induce changes among the particles of B, by destroying the statical condition, and forcing the particles into motion, whereupon they arrange themselves into new groups. This does not *explain at all the cases* included under the definition given in this dis-

course. It is just possible that it may ultimately prove to be a truth in relation to those cases, but it does not, *at present*, explain them.

3. Playfair observes, "There are many instances where catalytic decompositions ensue, where there is no intestine motion in the atoms of the exciting body. Hence we cannot do more than consider motion as favourable to the development of dormant affinities." He should, perhaps, have said, "Where there is no *apparent* intestine motion;" and there may be an undesirable ambiguity in the phrase, "favourable to the development of dormant affinities." In his essay on the subject, (a monument of learning and industry,) he concludes, that "the catalytic body is a substance which acts by adding its own affinity to that of another body, or by exerting an attraction sufficient to effect decomposition under certain circumstances, without being powerful enough to overcome new conditions, such as elasticity and cohesion, which occasionally intervene, and alter the expected result." (Thus, for instance, A and B have each separately an affinity for the element (*e*) of a third body E, but neither, separately, can tear it from its combination with E:—the joint attraction of A and B *may* be sufficient to release it; but the released element will not therefore necessarily combine with A and B.)

4. Faraday, and others, consider that many cases of catalysis are due to the powerful condensation of liquids, vapours, and gases, upon the surface of the catalyzing body. This view is of great importance, but not (as yet) equal to the explanation of nearly all the phenomena. Some of the arguments in favour of it may be found in a consideration of the probable mechanical condition of platinum, owing to the method of manufacture, the ascertained absorptive power of certain catalyzing agents, and the probable condensation of water in sulphuric acid.

5. De la Rive explains the particular case of the combination of hydrogen and oxygen on the surface of platinum, by supposing that the platinum is oxidated on its surface, and the oxide continuously reduced by the hydrogen, water resulting. (See Gmelin's Chemistry, published by the Cavendish Society, Vol. II. p. 56.)

Mr. Ashby then described his own researches on catalytic combustion. If the sesquioxides of chromium, nickel, cobalt, manganese, and iron, be laid upon wire-gauze (about 60 meshes to the inch-linear,) then warmed in the flame of a spirit-lamp, and laid over capsules nearly full of alcohol, pyroxylic spirit, ether, or other similar substances, they will burst into glow and catalyze the vapours which rise into them, as long as the supply continues. Warm Cr_2O_3 will inflame a jet of hydrogen in contact with air. The sesquioxide of iron is peculiarly available for operations on any scale, however large, and those specimens are to be preferred which have the least density. A catalytic lamp was exhibited, by which spirit or benzole may be *consumed* by the catalytic glow for laboratory

purposes. Coal-gas mixed with air may also be employed under the gauze upon which the oxide is distributed. Euchrome (dug from the estate of Lord Audley) is a cheap and useful catalyzing agent, when freed from its carbon.* A mixture of ten parts by weight of chlorate of potash, with one part of light and finely divided sesquioxide of iron, yields oxygen with entireness and facility, and has the additional advantage that (n) grains of the mixture represent very nearly (n) cubic inches of evolved oxygen. If we take the case of the sesquioxide of iron, we are able, *to some extent*, to show the modus operandi of the catalytic action; we can arrest the process at the half-stage, and then at leisure complete the other half. By heating $Fe_2\,O_3$ to redness, and quenching it in boiling alcohol (air being excluded), or by passing the vapour of alcohol over heated sesquioxide, we obtain two results; the alcohol is oxidated, and the $Fe_2\,O_3$ becomes $Fe_3\,O_4$ by deoxidation. The same follows by treating the sesquioxide with ammonia, gaseous or in strong solution. This black magnetic oxide (probably $Fe_3\,O_4 + H\,O$) remains unchanged by a red heat, if oxygen be not present; but if warmed in contact with air, it absorbs oxygen at a temperature far below redness, and returns into the original $Fe_2\,O_3$. It is clear, then, that during the catalysis, an intestine motion of the particles is going forward, and every portion of the sesquioxide is constantly reduced by the alcohol and reoxidated from the air. This was illustrated by a diagram and a cluster of coloured balls. Attention was then drawn to a singular fact, which may probably be referable to the catalytic action of the sesquioxide of iron. If the ferrocyanide of barium be heated until it ceases to give off ammonia, and then placed over alcohol, ammonia is again evolved in small quantity; but if the same experiment be tried with pure prussian blue, a large quantity of ammonia is formed by the contact with alcoholic vapour. Alumina ($Al_2\,O_3$) presents a singular phenomenon, hitherto unobserved; a pure specimen, of snowy whiteness, on being heated to redness, and exposed to the vapour of alcohol, becomes dark-grey, or black, and the vapour is oxidated; and this effect may be produced (with less ease) by ammonia. It seems that the sesquioxide of alumina has become a lower oxide, as in the case of iron, but cannot recover oxygen from contact with that gas.

Mr. Ashby then exhibited a "galvanic indicator," by which he hopes to prove satisfactorily, whether or not a galvanic current is set in motion during catalytic combustion. In conclusion, he drew attention to several points of interest connected with catalysis.

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* Mr. Arthur Church has observed that several of the chromates (copper, manganese, cobalt, &c.) after ignition, will freely catalyze alcoholic and other vapours.

1. The value of catalytic processes in various manufactures. Nitric oxide acts catalytically in the preparation of sulphuric acid of commerce; spongy platinum has been employed for the same purpose; and a patent has lately been granted for a similar use of the sesquioxide of iron. Spongy platinum is sometimes employed in Germany for the preparation of acetic acid from alcohol. The manufacture of ether from alcohol by sulphuric acid has already been noticed.

2. Considering that catalytic combustion by the sesquioxide of iron is first set up at a temperature far below redness,—that even a rusted nail may be sufficiently active,—and that in many cases of fermentation inflammable vapours may be disengaged, and much heat evolved,—it is not unreasonable to suppose that in this way we may account for some instances of what is commonly called "spontaneous combustion."

3. There is much reason to suppose that catalysis plays a great part in the organic chemistry of nature, in relation to vegetable and animal life. The experiments of Mr. Turnbull and Dr. Stenhouse point to catalysis as a vast sanitary agent. The dead bodies of various animals were covered with a layer of charcoal, which rather assisted decomposition than otherwise, serving as a carrier to the oxygen of the atmosphere, and delivering the perfectly innoxious and inodorous products of catalytic combustion into the air, while the charcoal itself remained entire and unconsumed. If we bear in mind (as established by Mulder and others) that humus has the same property, and that many oxides in the mould are catalytic in a greater or less degree, it becomes evident that interment underground may bring into play a catalytic process by which the elements of an organic body are returned into the atmosphere in forms which are not prejudicial to existing life.

4. Since some oxides exhibit a tendency to promote catalytic combustion, which are at present thought to be the only oxides of those bases, and in the case of one oxide (iron) the process has been shown to consist of alternate reduction to a lower oxide, and re-oxidation,—it is quite possible that further research may lead to the discovery of some new oxides.

[J. E. A.]

WEEKLY EVENING MEETING,

Friday, March 30.

WILLIAM ROBERT GROVE, ESQ. Q.C. F.R.S. Vice-President, in the Chair.

THE REV. JOHN BARLOW, M.A. F.R.S. Vice-President and Secretary R.I.

On the application of Chemistry to the Preservation of Food.

NEITHER force nor matter has been added to or taken from the natural world since the commencement of its present condition—consequently the development and growth of successive races of plants and animals are dependent on the supply of materials which once formed part of the structure of their predecessors. Needful, however, as is the decomposition of the dead to the continuance of universal life over the globe, it is equally necessary to the maintenance of civilized life, that this law should be modified. Man has been, accordingly, permitted to acquaint himself with the conditions of destruction so far as to become able to suspend its process. These conditions appear to be—1. A temperature above 32° Fah. 2. The presence of air and moisture. 3. A peculiar (generally liquid) condition of that albuminous substance which surrounds the cells and fibres of all animal and vegetable structures.

1. Decomposition will not occur at the temperature of freezing water. We believe that long before this planet was inhabited by any creature of intelligence capable of receiving the idea of time, bodies of animals, which became extinct before man existed, remained undecomposed, because they were imbedded in ice, or in frozen earth.* Meat and fish are transmitted from Archangel for sale at St. Petersburg. Provisions are also sent packed in ice from remote parts of Britain to London. The use of ice-houses and ice-chests for the preservation of food, is among many obvious applications of this principle.

2. Decomposition will not occur, if moisture be excluded.

* Pallas's Travels, quoted by Sir C. Lyell; "Principles of Geology," 8th Ed., p. 82.

The decomposing property of the oxygen of the air is most efficient where water is present in a state of minute subdivision. In this case the water (even if it does not promote corruption by itself becoming decomposed,) absorbs from the atmosphere oxygen, which it presents to the decomposable body in a state the most effective for its destruction. "Rain-water, and especially dew, will bring on the putrefaction of animal matters much sooner than spring-water; and the vulgar prejudice respecting the effect of the moon's rays in accelerating the corruption of meat, is, no doubt, dependent on the fact, that during clear moonlight nights, there is always a large deposition of dew; and this, having fallen in a minutely divided state, possesses the largest amount of free oxygen which pure or distilled water is capable of absorbing from the atmosphere; and therefore has a proportionate power of decomposing—just as it also has of bleaching." *

An interesting illustration of the respective effects of a dry and a moist atmosphere occurs in Sir F. B. Head's "Rough Notes." Sir Francis, contrasting the atmosphere of Mendoza, St. Lucie, and Buenos Ayres, (all being in the same latitude,) states that in the two former provinces, "The air is extremely dry: there is no dew at night, and the dead animals lie in the plain dried up in their skins, so that occasionally I have at first been scarcely able to determine whether they were alive or dead:" but in the province of Buenos Ayres, on sleeping out at night—"I have found my poncho, or rug, nearly wet through with dew; the dead animals in the plain are in a rapid state of putrefaction."†

3. Decomposition of a moist organic body may be prevented if air be entirely excluded.

"Gay-Lussac found that when bruised grapes were carefully excluded from the air, no change ensued; but that even a momentary exposure of the pulp to air or oxygen was sufficient to communicate to it the power of fermentation."‡—A bottle was exhibited, containing a piece of meat and a stick of phosphorus. They had remained together since January 15, 1855. The meat appeared fresh, but the phosphorus was visibly oxidized and acidified.

Application of these Principles to the Preservation of Food.

A. *Principle of desiccation at a temperature below that which would cause the albumen to coagulate.*

Animal and vegetable substances dried in the sun, or at a low

* On Preserved Meats for the Naval Service.—*Newton's London Journal*, Vol. xl. p. 200.

† "Rough Notes," &c. 2nd Ed. p. 8. Vide also on same subject, p. 142. Also Murray's "Handbook for Travellers on the Continent." 6th Ed., p. 268, on the preservation of the bodies of the monks at Kreuzberg.

‡ "Brande's Manual," Vol. ii. p. 1640. 6th Ed.

artificial heat, are less prone to decompose, even though they be not isolated from atmospheric air and moisture. Hay and dried fruits are obvious examples of the application of this principle.

B. ***Principle of isolation from atmospheric influences, the albuminous constituent remaining uncoagulated.***

An expedient for protecting meat on this principle has recently been carried out on a large scale at Paris. It is described at length in the *Assemblée Nationale* of the 23rd of January (quoted in the *Siècle* of the 22nd of February). The flesh is immersed in a gelatinous solution, which, when dry, forms an air-tight integument. It is named by the inventor "vernis cristallisé," and is said to make an excellent soup.—Joints of meat, both raw and fresh, and eggs, which had been prepared five months before by this process, were exhibited by Mr. M. Rennie: all appeared in good condition.*

If meat be coated with dry wheaten flour, the time that it will continue sweet may be prolonged threefold in tropical climates. The flour probably acts as an isolator against air and moisture.

The preservation of organic substances in glycerine, oil, and syrup, and the more familiar process of salting,† probably derive their efficacy from the principle of isolation.‡

But, although brine effectually protects the meat which it surrounds against atmospheric oxygen, "flesh by salting loses in point of nutritive value, in consequence of the removal and division of the salts indispensable to sanguification."§ This was illustrated by experiments on brine, which was obtained from a piece of beef salted in the course of the day, and proved to contain albumen, phosphoric acid, lime, and iron.

The following preparations made with glycerine, were exhibited—

(*a*) A small piece of flesh which had been partially immersed since February 20, 1855, in a bottle containing a small quantity of glycerine. By daily agitating this bottle, that part of the flesh which rose above the surface of the fluid was kept moist.

(*b*) In each of two jars of glycerine about 4 lbs. of meat were

* The specimen of raw flesh thus prepared had shrunk, as if the "vernis cristallisé" had acted as the skin of a grape when it becomes a raisin; *i.e.* it had permitted the escape of constituent moisture, but had prevented the admission of atmospheric moisture.

† On April 23rd, 1841, Mr. Macilwain gave an evening discourse on Payne's mode of salting, by subjecting the substance to atmospheric pressure.—*Literary Gazette*, for 1841, p. 280. *Repertory of Patent Inventions*, Vol. xvii. p. 168.

‡ It was shown, by placing vessels of glycerine and strong brine under the exhausted receiver, that the latter is almost entirely, the former absolutely, free from dissolved air.

§ Liebig's "Familiar Letters," p. 430. 3rd Ed.

‖ It has been suggested, that the sensation of thirst, experienced after eating salt meat, may, in some degree, be owing to the tendency of a saline solution to dry up animal membranes.

sunk by weights below the level of the fluid. This preparation was made on the 20th of February, 1845.*

In all these vessels the meat was fresh and sweet. The glycerine, however, like the brine, was reddened, indicating the separation of colouring matter, and therefore of iron, from the flesh immersed. The surface of the glycerine in the larger jars was studded with patches of mould. It was therefore inferred that some of the organic constituents of the meat, either singly or in combination, had been also separated; but that, owing to the impermeability of glycerine by air, the decomposition did not take place beneath the surface of the liquid.

In concluding this part of his subject, Mr. Barlow remarked, that one decomposition was sometimes had recourse to for the purpose of preventing another, as when wood, &c. is preserved by being painted over. The liquid paint is converted, by the action of the atmosphere, into a kind of solid, impermeable, insoluble soap. The stereochrome of Fuchs is another instance of this protective decomposition. There, however, the protection is derived from the combination of a portion of the protecting body with a part of that which it is intended to preserve.†

C. Organic Substances protected by a Change effected in their Albuminous Constituents.

This is accomplished

(1.) By chemical reagents.
(2.) By heat and desiccation.

(1.) Coagulation of albumen, *by chemical reagents.*

The preservation of timber, by corrosive sublimate, was fully described by Dr. Faraday, in a Friday evening discourse, in 1833, on the prevention of dry-rot, when it was proved that this salt formed a stable compound with the albuminous matter of the wood. On March 7, 1845, Mr. Goadby discoursed "on the nature and action of preserving fluids, as applied to animal structure."‡ This process consisted in the use of a solution of salt, with a small addition of corrosive sublimate.§ In illustration of this mode of preservation, Mr. Barlow exhibited specimens of flesh which had

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* The glycerine used in one of these two jars was prepared according to Tilghman's process, by the action of water at a high temperature. *Newton's London Journal*, Vol. xlv. p. 343. The glycerine contained in the other jar was obtained by the decomposition of a fatty body by steam, as described by George Wilson, Esq. *Proceedings of Royal Society*, Vol. vii. p. 182.

† Vide Proceedings of the Royal Institution, April 7, 1854. Vol. I., p. 424. (Rev. J. Barlow on Silica).

‡ Vide *Athenæum* for 1845, p. 272.

§ Vide Admiralty Manual of Scientific Enquiry, edited by Sir J. Herschel; article Zoology, by Professor Owen, p. 357; and "Directions for Preserving Specimens of Natural History," published by the Smithsonian Institute, Washington, U.S. 1854.

been immersed since December 1st, 1854, in alcohol, in pyroxylic spirit, and in fusel-oil, as well as other similar specimens, which had been suspended, for an equal period of time, in the vapours of the same liquids. The antiseptic effects of creosote, the active principle of wood-smoke, (which is almost the only substance whose chemical reaction on the albuminous constituent of organic matter is employed for the preservation of food,) were then noticed.

(2.) Coagulation of the albuminous constituent *by heat.*

The identity of the antiseptic effect of heat with that of a strongly coagulating reagent, as corrosive sublimate, was shown by exhibiting—(*a*) Two pieces of flesh, one of which had been thoroughly cooked and dried, and the other washed over with a solution of sublimate. Both had been kept dry: the surface of both was hard and dark. (*b*) Two similar pieces of flesh, similarly prepared, but exposed to a damp atmosphere: both were in a mouldy, but neither of them in a putrescent condition.

Dr. Verdeil has applied the coagulating effect of heat to the preservation of vegetables and of meat. He directs that green (*i. e.* unripe) vegetables, such as cabbages, carrots, green peas, French beans, should be submitted to the action of high-pressure steam of the temperature of about 300° Fah., and then dried as quickly as possible.

Specimens of vegetables prepared by this process were exhibited. Although kept in paper envelopes they appeared to have suffered no change from the effect of air. The aromatic oils being retained in the coagulated albumen, imparted its characteristic flavour to the cooked vegetable, which, retaining the minutest details of its structure, resumed its original form and size on being steeped in water.*

The same principle has been applied by Dr. Verdeil to the preservation of meat.

Edwards's potatoes, belonging to the same class of preparations, claim notice from the rapidity and facility with which they can be dressed.

D. The Principle of Coagulation of Albumen by Heat, but without Desiccation, the Provisions being isolated from Air and External Moisture.

This seems to be the principle of the well-known processes both of Appert and of Goldner. The former aims at the combination of whatever oxygen may be present in the vessel containing the pre-

* This was illustrated by the expansion of a cauliflower, an artichoke, and of Brussels sprouts, and by the odour and flavour of carrots, turnips, onions, &c. all prepared by Dr. Verdeil, which were cooked during the evening in the presence of the Members and visitors.

served provisions, with some of the organic matter; and the latter, at exclusion of that oxygen by the action of steam. A canister of rice and meat, prepared by Appert, which had been in the possession of Messrs. Fortnum and Mason* for ten years, was opened under a saturated solution of salt; the air it contained was collected in the usual way, and when tried by the nitric-oxide (NO^2) test, was found free from oxygen.

Bottles of choice fruit and green vegetables, in excellent condition, were exhibited; these had been prepared five or six years since by Appert's process. M. Appert kindly sent from Paris for exhibition on this evening a case of beef and soup, 3000 kilogrammes of which he is now daily preparing for the French army in the Crimea. M. Appert also exhibited specimens of more luxurious delicacies, as crême au chocolat, pâté de foies gras, pâté de canard.

The same process has been adopted and developed in this country, with eminent integrity and success, by Mr. Gamble, who has succeeded in preserving not only the substantial materials of food, but also fish and other luxuries of the table.†

Two large tin canisters of dressed meat, which had been prepared by Goldner's process (expulsion of air by steam) in the year 1840, in the presence of Dr. Faraday and Professor Graham, were exhibited. As neither of these canisters had bulged out, it was inferred that no gas had been formed, and that therefore no decomposition of their contents had taken place.

E. Preservation of Milk.

Four processes of preserving milk were noticed; each depended on a different principle.

1. *The process of M. Mabru.*

This process preserves milk without addition of any substance whatever. It consists essentially in the exposure of metallic bottles, each containing about a quart of milk, to steam raised to the temperature of 212° Fah. These bottles are fitted with leaden tubes, by means of which they were vertically suspended in the steam from a chest filled with milk, so that there was constantly a layer of milk above the extremities of the leaden tubes. After

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* [I am anxious to acknowledge the liberality with which Messrs. Fortnum and Mason put a large and valuable collection of specimens at my disposal, for experiment as well as for exhibition.—J. B.]

† Among the specimens of Mr. Gamble's process were calf's head with turtle-gravy, soup of ox-cheek, ox-tail, giblet, and mulligatawny, and mutton broth, stewed rump-steaks, mackerel, stewed eels, lobsters, tripe, cream, and butter. Some of these were opened and cooked during Mr. Barlow's discourse. A scientific interest has attached itself to these articles of luxury, as well as to those of M. Appert, because their preservation depends on the delicate adjustment of the heat employed in preparing them, between the temperature which would destroy their flavour, and that which would be insufficient to ensure their remaining uninjured in tropical climates.

having received sufficient heat, the bottles and their contents were suffered to cool, and, when cold, the leaden tubes were carefully closed under the surface of the milk, to prevent the admission of air. A bottle of milk thus prepared, which had been kept fourteen months, was opened, and found unaltered except by the separation of a small quantity of cream.*

2. *Process of M. Francis Bernard Bekaert.*

This method differs from that of M. Mabru in the addition of a few drops of solution of carbonate of soda to the milk before it is subjected to the boiling temperature. In this process the milk may be kept in glass bottles, which, however, must be carefully corked. After the weak alkaline solution has been added the whole is heated in water, gradually raised to the temperature of 212°, and afterwards is slowly cooled. A bottle of milk thus prepared was perfectly sweet and fresh after having been kept ten weeks.†

3. *Process of Mr. E. D. Moore.*

Mr. Moore removes from the milk its constituent water, retaining its component elements. The condition in which the butter, caseine, &c., are preserved is such, that when the paste comes to be again united with water, the milk reassumes its original appearance and flavour.

4. *Solidified Milk.*

By successive applications of carefully regulated heat, and by the addition of a substance which he has discovered, M. Fadeuilhe has succeeded in removing from the milk those of its constituents which, as he believes, cause it to decompose, and are also injurious to health. Sugar and a small quantity of gum tragacanth are then added to the residue, which is ultimately solidified by the prolonged action of a constantly varied temperature. Unlike the preparations of milk already described, the solidified milk of M. Fadeuilhe does not require to be kept out of contact of air. It is sent into the market in paper wrappers.

F. Meat-Biscuits.

These may be regarded as the full developement and scientific perfection of the pemican principle. Pemican is dried meat, thoroughly mixed with fat, sugar, or spice. Meat-biscuit consists of flour, baked with a solution of the nutritious ingredients of flesh,

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* A full description of the details of M. Mabru's process will be found in Cosmos, Vol. v. p. 325. It was to the good offices of the Abbé Moigno, the learned editor of that journal, that Mr. Barlow was indebted for this specimen of milk, as well as for the preserved provisions of M. Appert.

† The small quantity of air necessarily intervening between the cork and the surface of the milk in the bottle did not appear to have produced any effect.

separated from its fibre. It will keep, when dry, for an unlimited period. The Council-Medal, at the Great Exhibition of 1851, was awarded to the meat-biscuit of Gail Borden. "Ten pounds of this substance, with a proper allowance of water, afford, both in bulk and nutriment, food sufficient to support the physical and mental powers of a healthy working man for a month."*

A specimen of this meat-biscuit was exhibited, as was also a meat-cake prepared by M. Pouteau, of Bucharest, from flour and the soluble constituents of beef. Each cake affords three good meals, weighs 7½ ounces, divided into three rations of 2½ ounces each.†

The combinations of Chocolate with milk paste or solidified milk, respectively prepared by Messrs. Moore and Fadeuilhe, may be regarded as an important supplement to this class of preserved provisions. To these Dr. Bence Jones has made a valuable addition by his Extract of Tea. This extract, a specimen of which, prepared in the laboratory of the Institution, was exhibited, is made by evaporating a strong infusion of tea to dryness in a water-bath. If intended for ready use in travelling, it should be well mixed with twenty times its weight of sugar: a tea-spoonful of this preparation is sufficient to make a cup of tea.‡

[J. B.]

* Reports of the Juries, p. 65.

† O'Brien's Danubian Provinces, 2nd Edition.

‡ In his "Report to the Hudson's Bay Company," Dr. Rae refers to one of the islands, discovered in the expedition which he relates, having been named Bence Jones, "after the distinguished medical man and analytical chemist of that name, to whose kindness I and my party were much indebted for having proposed the use and prepared some extract of tea for the expedition." —*Household Words, February* 1855, p. 18.

Mr. Faraday brought before the Members a specimen of rolled and laminated Aluminium from Dr. Percy. It had been prepared by Mr. A. Dick, and was obtained by the direct action of sodium on cryolite,—which being a fluoride of aluminium and sodium is by more sodium resolved into aluminium and fluoride of sodium.

GENERAL MONTHLY MEETING,

Monday, April 2.

Aaron Asher Goldsmid, Esq.
in the Chair.

Eustace Anderson, Esq.
Andrew Whyte Barclay, M.D. and
Thomas Parry Woodcock, Esq.
were duly *elected* Members of the Royal Institution.

John Baily, Esq. Q.C.
Charles Beevor, Esq.
Henry Bradbury, Esq.
John Dickinson, Esq.
Rev. George Delgarno Hill.
Henry M. Noad, Esq. Ph.D. F.R.S.

were *admitted* Members of the Royal Institution.

A Special Vote of Thanks was returned to His Grace the Duke of Northumberland, the President, for his Present of a magnificently bound copy of Rosellini's "Monumenti dell' Egitto e della Nubia," in nine volumes 8vo, with an Atlas of Plates in three volumes folio.

The following Presents were announced, and the thanks of the Members returned for the same.

From
Her Majesty's Government (through Sir H. De la Beche)—Catalogue of Specimens (at the Museum of Practical Geology) illustrative of the Composition and Manufacture of British Pottery and Porcelain. By Sir H. De la Beche and T. Reeks. 8vo. 1855.
Actuaries, Institute of—The Assurance Magazine. No. XIX. 8vo. 1855.
Anderson, W. J. Esq. M.R.I. (the Author)—Continued Fever in Children. 12mo. 1854.
Astronomical Society, Royal—Monthly Notices. Vol. XIV. and Vol. XV. No. 4. 8vo. 1855.
Memoirs. Vol. XXIII. 4to. 1854.
Bayerische Akademie der Wissenschaften, die Königliche—Abhandlungen, Band VII. 2te Abtheilung. 4to. Munich, 1854.
Bulletin für 1853. 4to.
Magnetische Ortsbestimmungen in Bayern. Von Dr. J. Lamont. Theil I. 8vo. 1854.
Annalen der Königlichen Sternwarte bei München. Band VI. Von Dr. J. Lamont. 8vo. 1854.

Bradbury, Henry, Esq. M.R.I.—The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Part I. fol. 1855.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal, for March, 1855. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for March, 1855. 4to.
British Architects, Royal Institute of—Proceedings in March, 1855. 4to.
Civil Engineers, Institution of—Proceedings in March, 1855. 8vo.
East India Company, the Hon.—Catalogue of the Arabic, Persian, and Hindustány MSS. of the Libraries of the King of Oude. Compiled by A. Sprenger, M.D. Vol. I. 8vo. Calcutta, 1854.
Glossary of Judicial and Revenue Terms, and of Useful Words occurring in Official Documents relating to the administration of the Government of British India. By H. H. Wilson, M.A. F.R.S. 4to. 1855.
Editors—The Medical Circular for March, 1855. 8vo.
The Athenæum for March, 1855. 4to.
The Practical Mechanic's Journal for April, 1855. 4to.
The Mechanics' Magazine for April, 1855. 8vo.
The Journal of Gas-Lighting for April, 1855. 4to.
Deutsches Athenäum für März, 1855. 4to.
Everest, Rev. R. M.A. (the Author)—Journey through the United States and part of Canada. 8vo. 1855.
On Social Degradation. 8vo. 1855.
Faraday, Professor, D.C.L. F.R.S.—Monatsbericht der Königl. Preuss. Akademie, January und February, 1855. 8vo. Berlin.
Graham, George, Esq. Registrar-General—Report of the Registrar-General for March, 1855. 8vo.
Greenwich Royal Observatory—Astronomical, Magnetical, and Meteorological Observations in 1853. 4to. 1855.
Irish Academy, the Royal—Memoirs, Vol. XXV. Part 5. 4to. 1855.
Proceedings, Vol. VI. Part 1. 8vo. 1854.
Northumberland, Duke of, K.G. F.R.S. President R.I.—I Monumenti dell' Egitto e della Nubia dal Ippolito Rosellini. 9 vols. 8vo., and 3 vols. fol. Pisa, 1832-44.
Novello, Mr. (the Publisher)—The Musical Times for March, 1855. 4to.
Photographic Society—Journal, No. 28. 8vo. 1855.
Royal Society of London—Proceedings, Vol. VII. Nos. 9, 10. 8vo. 1855.
Radcliffe Trustees, Oxford—Astronomical Observations made at the Radcliffe Observatory, Oxford, in 1852. 8vo. 1854.
Snow, John, M.D. (the Author)—On the Mode of communication of Cholera. 8vo. 1855.
Society of Arts—Journal for March, 1855. 8vo.
Sykes, Col. F.R.S. (the Author)—Statistics of Nice Maritime. 8vo. 1855.
On the Miniature Chaityas found in the Ruins of the Temple of Sarnáth, near Benares. 8vo.
Address at his Installation as Lord Rector of the University, Aberdeen. 8vo. 1854.
Taylor, Rev. W. F.R.S. M.R.I.—Magazine for the Blind, March, 1855. 4to.
Webster, John, M.D. F.R.S. M.R.I.—General Reports of the Royal Hospitals of Bridewell and Bethlem, &c. for 1854. 8vo. 1855.

WEEKLY EVENING MEETING,

Friday, April 20.

William Robert Grove, Esq. M.A. Q.C. F.R.S.
Vice-President, in the Chair.

T. H. Huxley, Esq. F.R.S.

On certain Zoological Arguments commonly adduced in favour of the hypothesis of the Progressive Development of Animal Life in Time.

When the fact that fossilized animal forms are no *lusus naturæ*, but are truly the remains of ancient living worlds, was once fully admitted, it became a highly interesting problem to determine what relation these ancient forms of life bore to those now in existence.

The general result of inquiries made in this direction is, that the further we go back in time, the more different are the forms of life from those which now inhabit the globe, though this rule is by no means without exceptions. Admitting the difference, however, the next question is, what is its amount? Now it appears, that while the Palæozoic *species* are probably always distinct from the modern, and the *genera* are very commonly so, the *orders* are but rarely different, and the great *classes* and *sub-kingdoms* never. In all past time we find no animal about whose proper sub-kingdom, whether that of the *Protozoa*, *Radiata*, *Annulosa*, *Mollusca*, and *Vertebrata*, there can be the slightest doubt; and these great divisions are those which we have represented at the present day.

In the same way, if we consider the Classes, *e. g. Mammalia*, *Aves*, *Insecta*, *Cephalopoda*, *Actinozoa*, &c., we find absolutely no remains which lead us to establish a class type distinct from those now existing, and it is only when we descend to groups having the rank of Orders that we meet with types which no longer possess any living representatives. It is curious to remark again, that, notwithstanding the enormous lapse of time of which we possess authentic records, the extinct ordinal types are exceedingly few, and more than half of them belong to the same class—*Reptilia*.

The extinct ordinal Reptilian types are those of the *Pachypoda*, *Pterodactyla*, *Enaliosauria*, and *Labyrinthodonta;* nor are we at present acquainted with any other extinct order of Vertebrata. Among the *Annulosa* (including in this division the *Echinoder-*

mata,) we find two extinct ordinal types only, the *Trilobita* and the *Cystideæ*.

Among the *Mollusca* there is absolutely *no* extinct ordinal type; nor among the *Radiata* (*Actinozoa* and *Hydrozoa*); nor is there any among the *Protozoa*.

The naturalist who takes a wide view of fossil forms, in connection with existing life, can hardly recognise in these results anything but strong evidence in favour of the belief that a general uniformity has prevailed among the operations of Nature, through all time of which we have any record.

Nevertheless, whatever the amount of the difference, and however one may be inclined to estimate its value, there is no doubt that the living beings of the past differed from those of the present period; and again, that those of each great epoch, have differed from those which preceded, and from those which followed them. That there has been a succession of living forms in time, in fact, is admitted by all; but to the inquiry—What is the law of that succession? different answers are given; one school affirming that the law is known, the other that it is for the present undiscovered.

According to the affirmative doctrine, commonly called the theory of Progressive Development, the history of life, as a whole, in the past, is analogous to the history of each individual life in the present; and as the law of progress of every living creature now, is from a less perfect to a more perfect, from a less complex to a more complex state—so the law of progress of living nature in the past, was of the same nature; and the earlier forms of life were less complex, more embryonic, than the later. In the general mind this theory finds ready acceptance, from its falling in with the popular notion, that one of the lower animals, *e. g.* a fish, is a higher one, *e. g.* a mammal, arrested in development; that it is, as it were, less trouble to make a fish than a mammal. But the speaker pointed out the extreme fallacy of this notion; the real law of development being, that the progress of a higher animal in development is not through the forms of the lower, but through forms which are common to both lower and higher: a fish, for instance, deviating as widely from the common Vertebrate plan as a mammal.

The Progression theory, however, after all, resolves itself very nearly into a question of the structure of fish-tails. If, in fact, we enumerate the oldest known undoubted animal remains, we find them to be *Graptolites*, *Lingulæ*, *Phyllopoda*, *Trilobites*, and *Cartilaginous fishes*.

The *Graptolites*, whether we regard them as *Hydrozoa*, *Anthozoa*, or *Polyzoa*, (and the recent discoveries of Mr. Logan would strongly favour the opinion that they belong to the last division,) are certainly in no respect embryonic forms. Nor have any traces of *Spongiadæ* or *Foraminifera* (creatures unquestionably far below them in organization,) been yet found in the same or contempo-

raneous beds. *Lingulæ*, again, are very aberrant *Brachiopoda*, in nowise comparable to the embryonic forms of any mollusk; *Phyllopods* are the highest *Entomostraca;* and the *Hymenocaris vermicauda* discovered by Mr. Salter in the Lingula beds, is closely allied to *Nebalia*, the highest Phyllopod and that which approaches most nearly to the *Podopthalmia*. And just as *Hymenocaris* stands between the other *Entomostraca* and the *Podopthalmia*, so the *Trilobita* stand between the *Entomostraca* and the *Edriopthalmia*. Nor can anything be less founded than the comparison of the *Trilobita* with embryonic forms of *Crustacea;* the early development of the ventral surface and its appendages being characteristic of the latter; while it is precisely these parts which have not yet been discovered in the *Trilobita*, the dorsal surface, last formed in order of development, being extremely well developed.

The *Invertebrata* of the earliest period, then, afford no ground for the Progressionist doctrine. Do the Vertebrata?

These are cartilaginous fish. Now Mr. Huxley pointed out that it is admitted on all sides that the brain, organs of sense, and reproductive apparatus, are much more highly developed in these fishes than any others; and he quoted the authority of Prof. Owen,* to the effect that no great weight is to be placed upon the cartilaginous nature of the skeleton as an embryonic character. There remained, therefore, only the heterocercality of the tail, upon which so much stress has been laid by Prof. Agassiz. The argument made use of by this philosopher may be thus shortly stated:—Homocercal fishes have in their embryonic state heterocercal tails; therefore, heterocercality is, so far, a mark of an embryonic state as compared with homocercality; and the earlier, heterocercal fish are embryonic as compared with the later, homocercal.

The whole of this argument was based upon M. Vogt's examination of the development of the *Coregonus*, one of the *Salmonidæ;* the tail of *Coregonus* being found to pass through a so-called heterocercal state in its passage to its perfect form.† For the argument to have any validity, however, two conditions are necessary. 1. That the tails of the *Salmonidæ* should be homocercal, in the same sense as those of other homocercal fish. 2. That they should be really heterocercal, and not homocercal, in their earliest condition. On examination, however, it turns out that neither of these conditions holds good. In the first place, the tails of the *Salmonidæ*, and very probably of all the *Physostomi* are not homocercal at all, but to all intents and purposes intensely heterocercal: the chorda dorsalis in the Salmon, for instance, stretching far into the upper lobe of the tail. The wide difference of this structure from true homocercality is at once obvious, if the tails of the *Salmonidæ* be

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* Lectures on the Comparative Anatomy of the Vertebrata, pp. 146–7.

† Von Bär had already pointed out this circumstance in *Cyprinus*, and the relation of the fœtal tail to the permanent condition in cartilaginous fishes.—See his "Entwickelungsgeschichte der Fische," p. 36.

compared with those of *Scomber scombrus*, *Gadus æglefinus*, &c. In the latter, the tail is truly homocercal, the rays of the caudal fin being arranged symmetrically above and below the axis of the spinal column.

All M. Vogt's evidence, therefore, goes to show merely that a *heterocercal* fish is heterocercal at a given period of embryonic life; and in no way affects the truly homocercal fishes.

In the second place, it appears to have been forgotten that, as M. Vogt's own excellent observations abundantly demonstrate, this heterocercal state of the tail is a comparatively late one in *Coregonus*, and that, at first, the tail is perfectly symmetrical, *i.e.* homocercal.

In fact, all the evidence on fish development which we possess, is to the effect that Homocercality is the younger, Heterocercality the more advanced condition: a result which is diametrically opposed to that which has so long passed current, but which is in perfect accordance with the ordinary laws of development; the asymmetrical being, as a rule, subsequent in the order of development to the symmetrical.

The speaker then concluded by observing that a careful consideration of the facts of Palæontology seemed to lead to these results:

1. That there is no real parallel between the successive forms assumed in the development of the life of the individual at present, and those which have appeared at different epochs in the past; and

2. That the particular argument supposed to be deduced from the heterocercality of the ancient fishes is based on an error, the evidence from this source, if worth anything, tending in the opposite direction.

At the same time, while freely criticising what he considered to be a fallacious doctrine, Mr. Huxley expressly disclaimed the slightest intention of desiring to depreciate the brilliant services which its original propounder had rendered to science.

[T. H. H.]

A series of specimens of Aluminium, prepared by M. St. Claire Deville, in Paris, were laid upon the Library table by Dr. Hofmann. These specimens consisted of a medal, with the head of the Emperor Napoleon III., two bars, a watch wheel, and a piece of copper plated with Aluminium. A large piece of Tellurium, prepared by Dr. Löwe, of Vienna, was likewise exhibited by Dr. Hofmann.

WEEKLY EVENING MEETING,

Friday, April 27.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President, in the Chair.

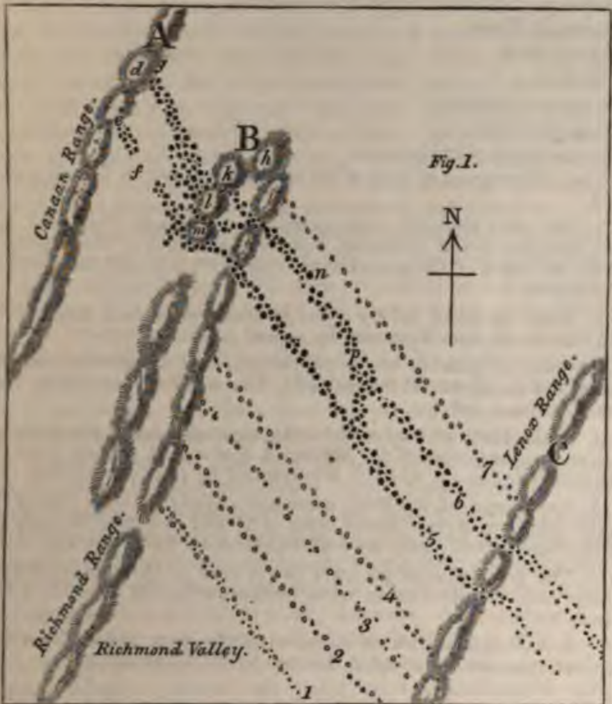
SIR CHARLES LYELL, F.R.S.

On certain Trains of Erratic Blocks on the Western borders of Massachusetts, United States.

ON the western borders of the State of Massachusetts, in Berkshire, and on the eastern confines of the adjoining State of New York, a great number of erratic blocks are seen, remarkable for their large size and their distribution in long parallel trains, each of them continuous in nearly straight lines over hill and dale, for the distance of five, ten, or twenty miles or more. These trains are of geological interest, not only from their length, and the size of the blocks, but also from the precision with which they can be traced to their starting points, and the low latitudes in which these starting points are situated. The area alluded to occurs in lat. 42° 25′ south, corresponding to that of the north of Portugal; and the western borders of Berkshire, where they join the State of New York, are about 130 miles from the Atlantic coast, in a direction due west of the city of Boston, in Massachusetts.

In the accompanying plan, Fig. 1, it will be observed that the mountain ranges A B C run N.N.E. and S.S.W., whereas the trains of erratic blocks (from No. 1 to No. 7 inclusive,) have a direction nearly transverse to these ranges, and consequently to the intervening valleys, their direction being about N.W. and S.E. In one sense we may affirm that the course of the stones has no relation whatever to the present configuration of the country, because the present drainage or flow of the rivers is quite in a different direction; but in another point of view we shall find that a close relation can be made out between the actual inequalities of hill and dale, and the course and mode of dispersion of the erratics; so that there is good reason to infer that the superficial inequalities were very nearly what they are now, before any of the trains originated.

Distance in a straight line between the mountain ranges A and C, about eight miles.



Map, showing the relative position and direction of seven trains of erratic blocks in Berkshire, Massachusetts, and in part of the State of New York.*

A. Canaan range, in the State of New York. The crest consists of green chloritic rock.

B. Richmond range (part of the Taconic range of Hitchcock?) the western division of which consists in Merriman's Mount of the same green rock as A, but in a more schistose form, while the eastern division is composed of slaty limestone.

C. The Lenox range, consisting in part of mica-schist, and in some districts of crystalline limestone.

* As the description of the trains could not be understood without a ground plan, the above copy of a diagram, exhibited during the discourse, is thought necessary; but it makes no pretensions to geographical accuracy, the speaker and his companion Prof. Hall having been unable to procure a good map of the district, on which to lay down in detail the results of their observations.

d. Knob in the range A, from which most of the train No. 6, is supposed to have been derived.

e. Supposed starting point of the train No. 5, in the range A.

f. Hiatus of 175 yards, or space without blocks.

g. Sherman's House.

h. Perry's Peak.

k. Flat Rock.

l. Merriman's Mount.

m. Dupey's Mount.

n. Largest block of train, No. 6.

p. Point of divergence of part of the train No. 6, where a branch is sent off to No. 5.

No. 1. The most southerly train examined by Messrs. Hall and Lyell, between Stockbridge and Richmond, composed of blocks of black slate, blue limestone, and some of the green Canaan rock, with here and there a boulder of white quartz.

No. 2. Train composed chiefly of large limestone masses, some of them divided into two or more fragments, by natural joints.

No. 3. Train composed of blocks of limestone and the green Canaan rock; passes south of the Richmond Station on the Albany and Boston railway; is less defined than Nos. 1 and 2.

No. 4. Train chiefly of limestone blocks, some of them 30 feet in diameter, running to the north-west of the Richmond Station, and passing south of the Methodist Meeting-house, where it is intersected by a railway cutting.

No. 5. South train of Dr. Reid, composed entirely of large blocks of the green chloritic Canaan rock; passes north of the Old Richmond Meeting-house, and is three-quarters of a mile north of the preceding train (No. 4).

No. 6. The great or principal train (north train of Dr. Reid), composed of very large blocks of the Canaan rock, diverges at *p*, and unites by a branch with train No. 5.

No. 7. A well-defined train of limestone blocks, with a few of the Canaan rock, traced from the Richmond to the slope of the Lenox range.

Dr. Reid, the agriculturist of Berkshire, first called attention in 1842 and 1845 to these phenomena. Professor Hitchcock contributed many valuable observations in 1844, and Professors Henry D. and William B. Rogers treated of the same subject in 1846.* The district was re-examined in October 1852, by Professor J. Hall and Sir Charles Lyell, by whom some of the data referred to in this discourse were ascertained. Within the area particularly referred to, the trains Nos. 5 and 6 are the most conspicuous, by their length and by the magnitude and frequency of the blocks composing them. These fragments consist of a green chloritic rock, remarkably tough, sometimes compact, but occasionally schistose. It is met with in place at *d*, in the highest crest of the Canaan ridge, and reappears in its more slaty form in the western division of the Richmond ridge B. at *l*, or Merriman's Mount. It passes on the one hand into a quartzose conglomerate,

* "Boston Journal of Natural History," for June 1846.

and on the other, when most metamorphic, into a crystalline rock, in which sometimes chlorite, sometimes hornblende and felspar are developed. A large proportion of the green fragments, in trains 5 and 6, have evidently come from the ridge A, and a large proportion of the whole from its highest summit *d*, upon which fragments often 30 feet in length may now be seen, some of them having probably constituted for years the exposed crest of the ridge, and having in that position acquired a smoothed and rounded outline so characteristic of the protuberances of hard rock in regions where erratics and glacial striæ abound. Such dome-shaped masses are called "roches moutonnées," on the borders of Swiss glaciers. Several of the fragments having this shape, and lying on the crest of A, have been slightly removed from their original position, as if just ready to set out on their travels. They are angular in their lower parts, where they exhibit such an outline as the jointed rock would possess if a great fragment fell from an undermined cliff.

To the westward of the ridge A no similar green blocks are to be found, not even a small number, such as we might have expected to roll down to the base of a hill having so steep a western declivity. It is evident, therefore, that the propelling power, whatever it was, acted exclusively in a south-easterly direction.

Dr. Reid has traced the train No. 5 for more than ten, and No. 6 for more than twenty miles to the south-east, crossing the Richmond and Lenox mountains B and C, and probably extending beyond the points to which they have already been followed. Messrs. Hall and Lyell found both trains extremely well-defined after they emerge from the Richmond range, but by no means so distinct in their passage over the first valley between A and B. A great number of blocks have collected at the base of *d*, Fig. 1, or the highest knob before alluded to of A, particularly around *g*, or Sherman's House. From this point to the Richmond range, a nearly continuous stream may be traced, and the blocks are seen to pass through a gap or depression, in the eastern division of the ridge B, between Flat Rock and Merriman's Mount, *k* and *l*. But when we attempt to follow the other train, No. 5, from its supposed point of origin *e*, (a spot about half a mile distant from *d* before alluded to,) we find at *f* an hiatus, not less than 175 yards long, where there are no erratics. This break is not caused by the stones having been used up for building, no such materials being observable in the walls enclosing fields, or in the farm-houses in the neighbourhood. A vast number of blocks seem to have crossed the valley in a direct line between A and B, and to have accumulated on the north-western slope of Merriman's Mount *l*, as well as to the south of it, around Dupey's Mount *m*; and they seem to have crossed the Richmond ridge by depressions both to the north and south of Dupey's Mount, those to the north proceeding westward to join the train No. 6. The number of large blocks lying on the west slope of Dupey's Mount, and many of them to the south of

the line which would connect the southern train, No. 5, with its supposed starting point *e*, is very great. One of these, 24 feet long, is poised upon another which is about 19 feet in length. The largest of all, composed like the rest of the green Canaan rock, lies on the west flank of Dupey's Mount, and is called "the Alderman." Dr. Reid measured it, and ascertained that it is above 90 feet in diameter, and not much under 300 feet in circumference. At some points about 40 or 50 blocks, the smallest of them larger than a camel, may be seen at once. Among the larger masses the best known, in consequence of its proximity to the Richmond Meeting-house, belongs to train No. 6, and is that marked *n* on the plan, Fig. 1. According to the measurement of Messrs. Hall and Lyell it is 52 feet long, by 40 in width, its height above the drift in which it is partially buried being 15 feet. At the distance of several yards occurs a smaller block, three or four feet in height, 20 feet long, and 14 broad, composed of the same compact chloritic rock, and evidently a detached fragment from the bigger mass, to the lower and angular part of which it would fit on exactly. This erratic (*n*) has a regularly rounded top, worn and smoothed like the roches moutonnées before mentioned, but no part of the attrition can have occurred since it left its parent rock, the angles of the lower portion being quite sharp and unblunted.

After the two great trains, Nos. 5 and 6, have crossed the ridge B, and entered the Richmond valley, which is about four miles broad, and about 800 feet deep below the crests of A and B, each train is exceedingly well defined. They are about half a mile apart, the train No. 6 varying in width from 100 to 300 feet, the space intervening between them usually very free from erratics, but here and there a solitary large straggler being visible. At one point *p*, Fig. 1, part of the train No. 6 diverges and forms a branch uniting with No. 5.

The average size of the blocks of all the seven trains laid down on the plan lessens sensibly in proportion as we recede from their point of departure, yet not with any regularity, a huge block recurring here and there in the midst of a train of smaller ones. Many which have wandered farthest from their parent rock retain their angles extremely fresh and sharp. Almost everywhere beneath the trains is a deposit of sand, mud, gravel, and stones, for the most part unstratified, and resembling the "northern drift" of Great Britain and parts of the north of Europe. It varies in thickness from 10 to 50 feet, being of greatest depth in the valleys. The uppermost portion is occasionally, though rarely, stratified; and where stratification occurs, it seems as if the mass first thrown down had been acted upon by currents, and partially rearranged. This drift has been well exposed in some recent railway cuttings, where it is occasionally seen to be 20 or 30 feet thick, immediately under several of the trains before alluded to. The stones in general are more rounded than the erraticsal ready described; occasionally some

are seen with one or more flattened, smooth, striated, or furrowed sides. They consist invariably, like the seven trains before mentioned, of kinds of rock only met with in the region lying to the north-west. In one cutting, the drift below the main train No. 6 is 30 feet thick, and contains one or two angular blocks of the green Canaan rock, of considerable, but not of the largest size. There are no appearances here or elsewhere warranting the conclusion that the trains owed their origin to the removal of an upper portion of the covering of drift, the lighter materials having been washed away, and the heavier made to stand out in relief. On the contrary, the erratics of each train, whether large or small, look as if they had dropped down over the linear spaces where they are now strewed, on the surface of hill and valley, equally where the drift is thickest, as where it is very thin or wanting. As a rule, the drift contains no blocks of the first magnitude, although a few occur, and some of the biggest are partially buried in drift, showing that the transport of the heavier, and of a certain portion of the lighter materials, was contemporaneous.

Almost in every place where the removal of the superficial detritus has exposed the underlying rock, a polished, striated, and furrowed surface is seen, like that underneath the modern glaciers of the Alps. The direction of the rectilinear furrows or grooves has been proved by a multitude of observations, made by Professor Hitchcock in this and other adjoining parts of Massachusetts and New York, to be from N.W. to S.E. or similar to the course of the large erratics.* The same geologist has pointed out that such ridges as A B and C are smooth and furrowed, not only on their tops, but sometimes 100 or 200 feet below, on their north-western sides; whereas, on their south-eastern declivities, if steep, there has been no such action, although these also are grooved and polished where their slope is gentle. The furrows, which are from an inch to a foot in depth, usually cross the strike of the highly inclined slates and slaty limestones at a considerable angle, and in such a manner as to demonstrate that the strata of the ridges A B and C, and of the intervening valleys, bent as they are into a series of anticlinal and synclinal folds, were already in their present position, and had even suffered aqueous denudation before the drift was thrown down upon them, and therefore long before the distribution of the erratics above described had taken place.

Although the trains, Nos. 5 and 6, are the most conspicuous, several of the others are well defined, and contain limestone blocks 30 feet in diameter. Thus No. 7 was seen on the western flank of the Lenox range, and followed across the Richmond valley to the eastern side of the ridge B, where the limestone, which has supplied the travelled masses, is in situ. In like manner, Messrs. Hall and Lyell observed, half a mile south of Pittsfield, enormous

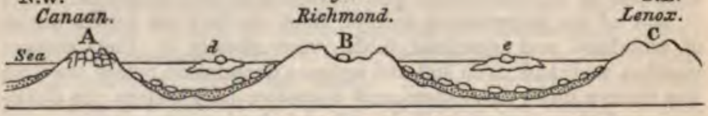
* "Final Report on Geology of Massachusetts," p. 383, *et seq.* 1841.

blocks of mica-schist, from 30 to 50 feet in their longest diameter, on the south-east side of the Lenox range C; whereas no similar fragments of mica-schist, whether large or small, are found in any part of the Richmond valley, or on the ridge B, or indeed anywhere between A and the Hudson river.

Some boulders of white quartz rock, two or three feet in diameter, make a part of almost every train, as well as of the subjacent drift, and these may be traced to hills, between the Canaan ridge and the Hudson river, where the Potsdam sandstone has been altered into quartzite.

Sir Charles then proceeded to explain his theory. He believes that all the large erratics have been transported to the places they now occupy by floating ice,—not by icebergs, nor by terrestrial glaciers, but by coast-ice. The hypothesis of glaciers is out of the question; because, even if we could imagine that in lat. 42° 30′ N., the ridges A B C, now only from 1000 to 2000 feet above the sea, once rose, and that at a period, geologically speaking, very modern, to such an elevation as to enable them to generate glaciers, still such glaciers could not have descended from the higher regions in one direction only. They would have radiated from a centre, carrying as many blocks westward as eastward. Their course, moreover, would have been principally S.S.W., or down the valleys now separating the ridges, instead of being south-east, or almost at right angles to the valleys. If, on the other hand, we assume that the country was lower instead of higher, so as to have been submerged beneath the waters of a sea, in which icebergs floated annually from arctic regions, these bergs might bring with them gravel and stones of northern origin, but could not without the aid of coast-ice become freighted with blocks derived from the very region referred to in this discourse, (lat. 42° N.) The northern ice might aid, by chilling the waters of the ocean, and increasing the quantity of coast-ice in a low latitude, but it could do little more.

Fig. 2.



d, e Masses of floating ice carrying fragments of rock.

Suppose the highest peaks of the ridges A B C, in the annexed diagram, to be alone above water, forming islands, and *d e* to be masses of floating ice which drifted across the Canaan and Richmond valleys, at a time when they were marine channels, separating islands, or rather chains of islands, having a N.N.E. and S.S.W. direction. A fragment of ice, such as *d*, freighted with a block from A, might run aground, and add to the heap of erratics at the N.W. base of the island B, or passing through a sound between B and the next

island of the same group, might float on till it reached the channel between B and C. Year after year two such exposed cliffs in the Canaan range as *d* and *e* (Fig. 1), undermined by the waves, might serve as the points of departure of blocks, composing the trains Nos. 5 and 6. It may be objected that oceanic currents could not always have had the same direction; this may be true, but during a short season of the year when the ice was breaking up the prevailing current may have always run S.E.

If it be asked why the blocks of each train are not more scattered, especially when far from their source, it may be observed, that after passing through sounds separating islands, they issued again from a new and narrow starting point; moreover, we must not exaggerate the regularity of the trains, as their width is sometimes twice as great in one place as in another; and No. 6 sends off a branch at *p*, which joins No. 5. There are also stragglers, or large blocks, here and there in the spaces between the two trains. As to the distance to which any given block would be carried, that must have depended on a variety of circumstances; such as the strength of the current, the direction of the wind, the weight of the block, or the quantity and draught of the ice attached to it. The smaller fragments would, on the whole, have the best chance of going farthest; because, in the first place, they were more numerous, and then being lighter, they required less ice to float them, and would not ground so readily on shoals, or if stranded, would be more easily started again on their travels. Many of the blocks, which at first sight seem to consist of single masses, are found, when examined, to be made up of two, three, or more pieces, divided by natural joints. In case of a second removal by ice, one or more portions would become detached and be drifted to different points further on. Whenever this happened the original size would be lessened, and the angularity of the block previously worn by the breakers would be restored, and this tendency to split may explain why some of the far-transported fragments remain so angular.

In the ravine between Merriman's Mount and Flat Rock (*k* and *l*, Fig. 1), the erratics, instead of following a N.W. and S.E. course, run within 10 or 15 degrees west and east; and Messrs. Hall and Lyell observed that the glacial furrows there on the exposed rocks below deviated in like manner from the normal direction, and were directed like the train of erratics nearly west and east. They were told that the like deflection, both of trains and furrows, was observable where Nos. 5 and 6 cross the Lenox range; and this deflection is so represented on the plan (Fig. 1); although the speaker had not an opportunity of verifying the fact. The direction of floating ice, when threading the sounds separating islands, would be governed by the shape of the land and the marine channels, and might be expected to differ from the direction of currents flowing in the open sea between chains of islands.

Sir C. Lyell endeavoured in 1842, when explaining the origin of

drift, boulders, and glacial furrows in the neighbourhood of the Falls of Niagara, both in Canada and in the State of New York,* to show that the whole region, with its elevated platforms and its valleys, had first gone down gradually, and had then been re-elevated during the glacial period. All geologists who are acquainted with Berkshire, Massachusetts, are agreed that the position of the erratics cannot be explained by the subsequent unequal elevation of the mountain ranges, as if B, for example, had been uplifted to a greater height than A, after the great boulders had been stranded on B. It is clear that the ridge B has intercepted many erratics on its north-western side, as it would do now, if submerged, and the blocks have chiefly crossed through gaps or depressions in B. The glacial furrows also are such as would be made on the fixed rocks, after they had already assumed their actual position, and when the present hills and valleys existed.

Although the principal mass of drift had accumulated before the trains, yet we see some of the biggest blocks partially buried in drift. This we might have expected, according to the hypothesis above suggested, for coast-ice, such as forms annually in the Gulf of St. Lawrence and along the coast of Labrador, does not merely bear away great stones but also mud, sand, and gravel.

The speaker exhibited a drawing of a large angular block of sandstone, about eight feet in diameter, which he and Mr. J. W. Dawson saw in 1852, stranded on the mud-flats near the mouth of the Petitcodiac estuary, where it joins the Bay of Fundy, and where the water is salt. The ferrymen stated that this block was brought down by ice three years before, from a cliff several miles up the river. About the year 1850 much larger blocks of sandstone were removed by coast-ice from the base of a perpendicular cliff at the South Joggins, in the Bay of Fundy, in salt water, and floated for about half a mile, where they dropped or were grounded on one side of the pier built for loading vessels with coal. The vessels being thereby prevented from nearing the pier it was found necessary to blast these ice-borne rocks, at low tide, with gunpowder. All this occurred in latitude 46° N., corresponding to that of Bordeaux; and when we bear in mind that the Bay of Fundy opens towards the south, and is therefore never invaded by icebergs, such as are stranded occasionally near St. John's, Newfoundland, or such as are annually drifted far to the south of the Bay of Fundy in the open ocean, we may well imagine that, with a different configuration of the land, coast-ice may once have exerted great power as far south as the latitude of Berkshire. The buoyant power of sheets of ice, even of moderate thickness, is so great that the magnitude of erratics depends more on the dimensions of the fragments into which rocks undermined by the sea happen to split, than on the peculiar intensity of the frost in that region.

* "Travels in North America," Vol. I., p. 99; and Vol. II., p. 48.

It has been objected to the theory of submergence that the great train, No. 6, has climbed a part of the ridge B, higher than its supposed starting point in A. But there are no exact barometrical or trigonometrical data for this assertion. Messrs. Hall and Lyell could only estimate roughly the relative heights of the knob *d* in A, and that of the pass between *k* and *l* in B, by means of a spirit-level, as they stood on the Canaan knob. It appeared to them that the gap in B, or the ravine between Merriman's Mount and Flat Rock, is 50 or 100 feet below the highest crest of A, in which case the objection falls to the ground. Some of the erratics of No. 6, in the Richmond valley, have probably come from an elevated point in Merriman's Mount, and therefore present no difficulty, since that mount consists of the green chloritic rock, but others have come from the Canaan ridge, and have crossed the ridge B, as before stated. If it could be shown that some of these stones repose on B, at points actually higher than the crest of A, the fact would be important, but by no means inexplicable by the glacial theory. Mr. C. Darwin has shown, that during the gradual subsidence and submergence of a coast situated in high latitudes, packed ice with stones frozen into it is continually driven up on the sea-beach above high-water mark, and if the land be going down at the rate of a few inches in a year, the boulders, by being simply kept up to the sea level, will slowly climb up the hills higher and higher, so that a ridge after sinking may, when it re-emerges, have stones lodged upon it at levels above those of the lands from which the same stones were derived.*

The drift of Berkshire and of New England in general has a great resemblance to the terminal moraines of glaciers, being unstratified and containing fragments of rock, some angular, others rounded. But the proportion of rounded boulders is far more considerable in the drift than in an ordinary glacier moraine, in which last, as Mr. D. Sharp has lately shown in reference to some Swiss glaciers, the rounded blocks are quite the exception to the rule. Want of stratification is the natural result of the melting of matter out of stationary ice, the light particles and the heavy stones dropping down together, and no current of water sorting the materials, and carrying those of less specific gravity to greater distances. Stones frozen into coast-ice may have been rounded, some by rivers, others by the waves of the sea. The dearth of marine shells is sometimes urged as an argument against the hypothesis of submergence beneath the sea, and it is certainly strange that marine shells should be so rare in drift. They are, however, extremely scarce in parts of New England, such as Vermont and Maine, where a few, nevertheless, do occasionally occur; and this holds good in Canada, as also in Ireland and other parts of the North of Europe. As we cannot doubt that the formation accu-

* C. Darwin, Geol. Quart. Journ. Vol. VI., p. 315. 1848.

mulated in certain cases under water, we are at liberty to assume the same in all others, where such an explanation will best accord with the facts.

The Saxicava rugosa, Mya truncata, Natica clausa, and other recent species of mollusca, are common to the drift of North America and Europe, and constitute part of a fauna, characteristic of a climate somewhat colder than that of the latitudes where the fossils are now met with. The Caplan also, or Mallotus villosus, a fish swarming in the seas of Greenland, Labrador, and wherever ice abounds in the North Atlantic, is found fossil in clayey concretions or claystones, in the drift of Maine and Canada, as well as in Greenland. It appears that in the glacial period, as now, the isothermal lines, or rather the lines of equal winter temperature bent many degrees farther south on the west than on the east side of the Atlantic, so that the monuments of glacial action extend some eight or ten degrees farther south in North America than they do in Western Europe. Large erratics and glacial furrows are rare south of latitude 48° or 50° in Europe, and are seldom seen south of 40° in North America; but where mountain chains like the Alps or Himalaya have formed independent sources of cold, we find exceptions. In Madeira and the Canary Islands, between latitudes 28° and 33° N., Sir C. Lyell searched in vain for glacial striæ, and other concomitant phenomena, although some of the mountains there are of great height. In the southern hemisphere all the manifestations of the agency of ice, which are wanting in the equatorial zone, reappear in full force, when we reach Chiloe, Patagonia, and Tierra del Fuego.

If ice-islands, running aground on the bed of a sea or on a a coast, can smooth and furrow the subjacent floor of the ocean by pushing before them or pressing down under them loose sand, pebbles, and stones, the size of such islands is certainly sufficient to afford as much friction and mechanical force as we require. Some of them, measured in the southern hemisphere, exceed 10 miles in diameter, and their height out of the water is from 100 to 200 feet, the mass of ice below being about eight times in volume that rising above the sea-level; if such masses when they run aground are moving only at the rate of one or two miles an hour, their momentum must be enormous.

The area now subject to the action just alluded to in both hemispheres, is many times greater than that over which terrestrial glaciers descend. In like manner, in the period of the "Northern drift," the submarine were far more extensive than the supramarine glacial operations; and since we have evidence of much sea having been converted into land since the glacial period, we must expect to find more space bearing the imprint of subaqueous ice than of space exhibiting evidence of the movement of ice over dry land.

In conclusion, Sir C. Lyell observed, that as the great majority

of mankind live now as they have always lived, near the equator, or in countries not more than 25 degrees distant from it, they can never behold ice or snow. We may imagine, therefore, a nation to have made considerable progress in science without knowing anything of the causes appealed to in this discourse. If such a people were told by travellers of the geological appearances above described, how great would be their perplexity! They might at first ascribe the transport of erratics to floods of extraordinary violence, but they would scarcely be able to hazard a reasonable conjecture in regard to the coincidence between the direction of glacial furrows and that of the trains of erratics. A stone, not held fast by ice, but merely pushed along in mud, could not scoop out a long rectilinear furrow, one inch, or sometimes a foot deep, in a hard rock. Still more mysterious would be the discovery of a connection between the former migration southwards of an arctic fauna, and the conveyance of large erratics to the same regions. If the glacial hypothesis afforded no more than a plausible explanation of the association of so many distinct and independent classes of phenomena, it would deserve greater favour than has been shown to it by some modern geologists. The inclination evinced by many to introduce catastrophic action, as peculiarly applicable to the case of drift, arises in a great degree from the absence of stratification in drift. The usual geological proof of successive accumulation, and of the lapse of time, is here wanting; hence the sudden uplifting and sinking of land, the displacement of the sea, and the raising of gigantic waves of translation, rolling over continents at the rate of fifty miles an hour, accompanied by rapid gyrations of the marine fluid, have been imagined. The rate of movement, suggested by the glacial hypothesis, is singularly opposed to these views. Blocks carried by glaciers travel for centuries at an average rate of less than an inch per hour, and those borne along by floating ice float a mile or a mile and a half an hour. The observer of icebergs can seldom tell whether their course be north or south, east or west. In like manner the submergence and re-emergence of land will best account for the appearances above described, if the movement were slow, as now in Greenland and Scandinavia; in other words, if it be such as would be insensible to any human inhabitant. Yet the power of the machinery appealed to in both cases is equally vast; for it must be capable of uplifting and depressing continents, and removing from place to place the great volume of superficial materials found in the drift. The real difference of opinion consists in the amount of time during which the force is supposed to have been developed.

[C. L.]

ANNUAL MEETING,

Tuesday, May 1.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President, in the Chair.

The Annual Report of the Committee of Visitors was read, and adopted.—It states that the Contributions from Members and Annual Subscribers in 1854 were very satisfactory, as well as the Receipts for attendance at the courses of Lectures. The General Income exceeded the Expenditure of the year by the sum of £795. 2*s*. 4*d*.; and the Managers were enabled, in addition to the annual investment of the Accumulating Funds, amounting to £184. 10*s*. 1*d*., to lay out £500 in the purchase of £3 per cent. Consols, and to buy an Exchequer Bill for £100.

A List of Books Presented accompanies the Report, amounting in number to about 175 volumes, and making a total, with those purchased by the Managers and Patrons, of nearly 900 volumes (including Periodicals) added to the Library in the year.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to Professor Faraday, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—The Duke of Northumberland, K.G. F.R.S.

TREASURER—William Pole, Esq. M.A. F.R.S.

SECRETARY—Rev. John Barlow, M.A. F.R.S.

MANAGERS.

William H. Blaauw, Esq. M.A. F.S.A.
Sir Benjamin Collins Brodie, Bart. D.C.L. F.R.S.
John Bate Cardale, Esq.
Thomas Davidson, Esq.
George Dodd, Esq. F.S.A.
Sir Charles Fellows.
Aaron Asher Goldsmid, Esq.
Sir Henry Holland, Bart. M.D. F.R.S.
Henry Bence Jones, M.D. F.R.S.
George Macilwain, Esq.
Right Hon. Baron Parke, M.A.
John Percy, M.D. F.R.S.
Lieut.-Gen. Sir George Pollock, G.C.B.
Alfred S. Taylor, M.D. F.R.S.
Charles Wheatstone, Esq. F.R.S.

VISITORS.

Henry Browning, Esq.
John Charles Burgoyne, Esq.
John Robert F. Burnett, Esq.
Alexander Crichton, Esq.
Hugh W. Diamond, M.D. F.S.A.
Edward M. Foxhall, Esq.
Thomson Hankey, jun. Esq. M.P.
Admiral Sir T. Herbert. K.C.B. M.P.
John Hicks, Esq.
John Holdship, Esq. M.A.
Octavius Morgan, Esq. M.P. F.R.S.
Robert R. I. Morley, Esq.
John North, Esq.
Rev. Cyril W. Page.
Rev. William Taylor, F.R.S.

The President nominated the following Vice-Presidents for the ensuing year:—The Treasurer, the Secretary, Sir B. C. Brodie, Bart., Sir Charles Fellows, Bart., Sir Henry Holland, Bart., Right Hon. Baron Parke, and Sir George Pollock, G.C.B.

WEEKLY EVENING MEETING,

Friday, May 4.

SIR CHARLES FELLOWS, Vice-President, in the Chair.

DR. J. H. GLADSTONE, F.R.S. M.R.I.

On Gunpowder, and its substitutes.

THE object of the speaker was to return an answer to a question which had of late been frequently proposed to him, and no doubt to other chemists also:—"Cannot you now invent something much better than gunpowder? Are not some of your fulminating compounds much more powerful? Why should we still be using a substance which was discovered long before chemistry was a science?" Dr. Gladstone stated that some of his friends had considered him peculiarly qualified to give a reply to the query, since he had analyzed in turn the most terrible explosives with which modern science has made us acquainted; yet he confessed he laboured under a disadvantage in having no practical acquaintance with gunnery, nor even with those experiments by which the propulsive force of different explosives is tested. He could give no categorical answer to the proposed inquiry. He could point to no substance, and say of it—"For use in fire-arms this is decidedly superior to gunpowder;" nor was he willing to say—"No; it is beyond the power of our science to invent anything better." He was desirous of laying before the audience some of the principles upon which a judgment might be formed; of indicating the manner of investigation, as much as the results already arrived at.

In so doing he glanced first, in a cursory manner, at the various kinds of explosives with which chemists are acquainted. Any great and sudden increase of volume may give rise to the phenomena designated explosion; but such great and sudden increase never takes place by the mere dilatation of a solid or liquid body: it is always necessary that gases should be formed. The simplest form of explosion is when a liquid is suddenly converted into a gas either by the removal of pressure, or by the bursting of the vessel in which it was contained, as illustrated by the common "candle-cracker." The enormous expansion of a gas by the removal of pressure is taken advantage of for the projection of missiles in the air-gun, and in Perkins's steam-gun. In these cases there is no chemical change; but usually an explosion is the result of a rapid chemical action between the different constituents of a mixture, or chemical compound, by which substances are produced that occupy a very much larger space than the original combination did. Such an explosion is always attended with heat, and generally with light and noise. The substance exploded may be a mixture of two or more gases: for instance, if the fire-damp of the mines be set fire to in the air, it burns quietly with a luminous flame; if, however, it be previously mixed with air, on being ignited the flame passes instantly throughout the whole mass; and if mixed with twice its volume of oxygen, this takes place with great violence, and a loud report. One atom of the carburetted hydrogen combines with four atoms of oxygen, to form carbonic acid and water. In this case, however, the gases produced by the explosion would actually have occupied less space than the original mixed gases, and a positive contraction would have ensued, had it not been for the high temperature at which they were formed. In order to obtain very great expansion we must not start with a gaseous mixture. Solid or liquid oxygen is a desideratum, but it can be procured in that condition only when in a state of combination. There are several salts which contain a large quantity of this element, and which give it up with great facility —the nitrates and chlorates of potash or soda, for instance; and these salts contain also another element, which when free assumes a gaseous condition, even at ordinary temperatures.

Dr. Gladstone then proceeded to show the violent combustion that ensued when wood was thrown into one of these salts in a fused condition, and to demonstrate the still greater effects that resulted when the salt and the combustible had been previously mixed. He then rapidly described the manufacture of gunpowder from nitre, charcoal, and sulphur, and the different proportions of the three ingredients that are employed in different countries. In exploding, gunpowder produces carbonic acid and nitrogen gases, and sulphuret of potassium, which is also dissipated by the great heat evolved, and if it reach the air is converted into sulphate of potash, giving rise to the white smoke that follows the explosion. Beside these gases some others are always produced in small and varying quantity.

By burning a fuse under water these gases were exhibited. It is supposed that, at the moment of explosion, the heated gases occupy fully two thousand times the volume of the original powder. By mixing different combustibles with nitre various effects may be produced on explosion; sometimes the light, sometimes the heat, and sometimes the noise being the most remarkable. When nitre was an article of scarcity in France, the French chemists made many experiments with a mixture of chlorate of potash, charcoal, and sulphur; but this compound, though a good explosive, has several disadvantages, which have prevented its ever coming into extensive use. A white gunpowder has more recently been prepared by mixing chlorate of potash with yellow prussiate of potash and sugar.

The explosives hitherto described are all mixtures. There exist substances which contain all the elements of combustion within themselves, and which require only a slight elevation of temperature, or a smart blow, to alter their state of chemical combination, and suddenly to produce gaseous bodies in large quantity. Pre-eminent among these is Gun-cotton: a substance formed by immersing cotton in a mixture of nitric and sulphuric acids. It is generally allowed now that this compound consists of lignine, $C_{24}\,H_{20}\,O_{20}$, in which a portion of the hydrogen has been replaced by NO_4; difference of opinion exists as to the amount so displaced, but Dr. Gladstone had found it to be five atoms in the most explosive gun-cotton, three in that of inferior quality, which he designated cotton-xyloidine. The most explosive compound produces a sudden flash, but no smoke or loud noise, and leaves no residue whatever. Hydrocyanic acid is among the resulting gases.—Nitroglycerine, a liquid produced in a similar manner from glycerine, is of so explosive a nature, that if a single drop be struck by a hammer on an anvil it gives rise to a deafening report. Its composition is $C_6\,H_5\,3\,(NO_4)\,O_6$. Similar to this is nitromannite, which also explodes on percussion. Several other simple nitric acid substitution products are also capable of explosion; and so are certain salts of organic acids, which are analogous in their constitution; for instance, carbazotate of potash. Fulminating mercury and silver are also salts of an organic acid, the fulminic, which contains both oxygen and nitrogen. They explode, as is well known, by percussion, and with extreme violence. There are, however, certain detonating compounds, which contain no oxygen, nor any other supporter of combustion, but which are easily caused to undergo an internal change, and to resolve themselves into gaseous products. The most remarkable of these are certain substitution products of ammonia—the so-called ammoniurets of gold and other noble metals, and the so-called iodide and chloride of nitrogen. The iodide is a black powder, which when dry will explode on the slightest touch of a hard substance, and even sometimes by a sudden concussion of the air near it. Its composition had been examined by the speaker, and found

that the Government had lately organized the means of examining the merits of every suggested improvement, and that the appointed parties were now actively engaged in the investigation. At present there appear two improvements in the art of war, in which chemical science may be of service: the one in making shells, which shall burst upon striking—about which there is no chemical difficulty; the other in charging shells with substances that will give forth quantities of poisonous gas; a subject which has lately attracted much attention. It is to be hoped, that not only mechanical, but also chemical science, will soon furnish us with improvements on the present means of carrying on the war in which we are now engaged.

[J. H. G.]

GENERAL MONTHLY MEETING,

Monday, May 7.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

George Ade, Esq., and
William Stuart, Esq.
were duly *elected* Members of the Royal Institution.

Andrew Whyte Barclay, M.D.
was *admitted* a Member of the Royal Institution.

The following Professors were unanimously re-elected:—

William Thomas Brande, Esq. D.C.L. F.R.S. L. & E., as Honorary Professor of Chemistry in the Royal Institution.

John Tyndall, Esq. Ph.D. F.R.S., as Professor of Natural Philosophy in the Royal Institution.

The following Presents were announced, and the thanks of the Members returned for the same:—

From
Allies, Thomas W. Esq. M.A. M.R.I. (the Author)—St. Peter, his Name, and his Office, as set forth in the Holy Scriptures. 8vo. 1852.
Journal in France, in 1845 and 1848; with Letters from Italy, in 1847. 12mo. 1849.
The Royal Supremacy, viewed in reference to the two Spiritual Powers of Order and Jurisdiction. 8vo. 1850.

Asiatic Society of Bengal—Journal, No. 245. 8vo. 1854.
Astronomical Society, Royal—Monthly Notices. Vol. XV. No. 5. 8vo. 1855.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for April, 1855. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for April, 1855. 4to.
Bradbury, Henry, Esq. M.R.I.—The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Part 2. Fol. 1855.
British Architects, Royal Institute of—Proceedings in April, 1855. 4to.
Chemical Society—Quarterly Journal. No. 29. 8vo. 1855.
Civil Engineers, Institution of—Proceedings in April, 1855. 8vo.
East India Company, the Hon.—Rig-Veda-Sanhitá. Ancient Hindu Hymns. Translated by H. H. Wilson, M.A. F.R.S. 2 vols. 8vo. 1850–4.
Magnetical and Meteorological Observations at Bombay, in 1851. 4to. 1854.
Editors—The Medical Circular for April, 1855. 8vo.
The Athenæum for April, 1855. 4to.
The Practical Mechanic's Journal for May, 1855. 4to.
The Mechanics' Magazine for April, 1855. 8vo.
The Journal of Gas-Lighting for April, 1855. 4to.
Deutsches Athenäum for April, 1855. 4to.
Faraday, Professor, D.C.L. F.R.S. (the Author)—Experimental Researches in Electricity. Vol. III. 8vo. 1855.
Monatsbericht der Königl. Preuss. Akademie, März, 1855. 8vo. Berlin.
Frost, Charles, Esq. F.S.A. (the Author)—Notices relative to the Early History of Hull. 4to. 1827.
Franklin Institute of Pennsylvania—Journal, Vol. XXIX. Nos. 2, 3. 8vo. 1855.
Graham, George, Esq. Registrar-General—Report of the Registrar-General for April, 1855. 8vo.
Highley, Mr. (the Publisher)—Quarterly Journal of Public Health, and Transactions of the Epidemiological Society of London. No. 1. 8vo. 1855.
Hood, Charles, Esq. F.R.S. (the Author)—A Practical Treatise on Warming Buildings by Hot Water; on Ventilation, &c. 8vo. 1855.
King, Arthur J. Esq. (the Author)—How to learn Latin: or Artificial Memory applied to Latin Words. 12mo. 1855.
Madras Literary Society—Madras Journal. Nos. 10, 11. 8vo. 1836.
Barometrical Sections of India. By E. Balfour. fol. 1853.
Novello, Mr. (the Publisher)—The Musical Times for April, 1855. 4to.
Percy, John, M.D. F.R.S. M.R.I. (the Author)—Experimental Inquiry concerning the presence of Alcohol in the Ventricles of the Brain, after poisoning by that liquid. 8vo. 1839.
Perigal, H. jun. Esq. (the Author)—Perigal's Contributions to Kinematics, Bicircloids, &c. in six sheets. 1854.
Photographic Society—Journal, No. 29. 8vo. 1855.
Prince, C. Leeson, Esq. (the Author)—Results of a Meteorological Journal kept at Uckfield, Sussex, in 1854. Sheet.
Society of Arts—Journal for April, 1855. 8vo.
Taylor, Rev. W. F.R.S. M.R.I.—Magazine for the Blind, April, 1855. 4to.
Wegweiser zur Bildung für Deutsche Lehrer. Von Adolph Diesterweg. 2 vols. Essen, 1849–50.
Vereins zur Beförderung des Gewerbfleisses in Preussen.—Verhandlungen, Jan. und Feb., 1855. 4to. Berlin.
Weale, John, Esq. (the Publisher)—Rudimentary Treatises. 12mo.:—
Manual of the Mollusca. By S. P. Woodward. Part II. 1854.
Art of Photography. By Dr. G. C. Halleur. 1854.
Form of Ships and Boats. By W. Bland. 1852.
Arithmetic, and Key. By J. R. Young. 1853–4.
Educational Series. 12mo.
History of England. By W. D. Hamilton. Vols. III. and IV. 1854–5.
History of Greece. By W. D. Hamilton and E. Lieven. 2 vols. 1854.
History of Rome. By E. Lieven. Vol. I. 1854.

Weale, John, Esq. (the Publisher)—Educational Series. 12mo.:—
Chronology of History, Art, and Literature. Vol. I. 1854.
Dictionary of the English Language. By Hyde Clarke. 1855.
Grammar of the Greek Language. By H. C. Hamilton. 1854.
Lexicon of the Greek Language. By H. R. Hamilton. Part II. 1853.
English-Greek Lexicon. By H. R. Hamilton. Part I. 1855.
Grammar of the Latin Tongue. By T. Goodwin. 1854.
Latin-English Dictionary. By T. Goodwin. 1855.
French-English, and English-French Dictionary. By A. Elwes. 2 Parts. 1854–5.
Italian-English-French Dictionary. By A. Elwes. Part I. 1855.
Spanish-English, and English-Spanish Dictionary. By A. Elwes. 1854.
Hebrew and English Dictionary and Grammar. By M. H. Bresslau. 1855.
Classical Series. 12mo. (Edited by H. Young.)
Greek Delectus. 1854.
Xenophon's Anabasis. 2 vols. 1854–5.
Select Dialogues of Lucian. 1855.
Latin Delectus. 1854.
Cæsar's Commentaries. 1854.
Cornelius Nepos. 1855.
Virgil. Part I. 1855.

WEEKLY EVENING MEETING,

Friday, May 11.

Sir Charles Fellows, Vice-President, in the Chair.

Henry Bradbury, Esq. M.R.I.

On Nature-Printing.

The Art of Nature-Printing is a method of producing impressions of plants and other natural objects, in a manner so truthful that only a close inspection reveals the fact of their being copies. So distinctly sensible to the touch are the impressions, that it is difficult to persuade those unacquainted with the manipulation, that they are the production of the printing-press. The process, in its application to the reproduction of botanical subjects, represents the size, form, and colour of the plant, and all its most minute details, even to the smallest fibre of the roots.

The distinguishing feature of the process, compared with other modes of producing engraved surfaces for printing purposes, consists, firstly, in impressing natural objects—such as plants, mosses, seaweeds, feathers, and embroideries—into plates of metal, causing as it were the objects to engrave themselves by pressure; and secondly, in being able to take such casts or copies of the impressed plates as can be printed from at the ordinary copperplate-press.

SPECIMEN.

NATURE PRINTING.

This secures, on the one hand, a perfect representation of the characteristic outline of the plant, as well as that of some of the other external marks by which a plant is known, and even in some measure its structure, as for instance in the venation of ferns, and the leaves of flowering plants ; and on the other, affords the means of multiplying copies in a quick and easy manner, at a trifling expense compared to the result obtained—and to an unlimited extent.

The great defect of all pictorial representations of botanical figures has consisted in the inability of art to represent faithfully those minute peculiarities by which natural objects are often best distinguished. Nature-Printing has therefore come to the aid of this branch of science in particular, whilst its future development promises facilities for copying other objects of nature, the reproduction of which is not within the province of the human hand to execute ; and even were it possible, it would involve an amount of labour scarcely adequate to the results obtained.

Although considered for some years past in various parts of Europe as a new art, the idea is by no means so recent as is supposed : much less is there ground for the Austrians to assert their exclusive right to the priority of the invention, merely on account of the first application of the process in its fullest extent in the Imperial Printing-Office at Vienna.

Councillor Auer * has not only done this, but he has claimed for Nature-Printing a position to which it has no right : he has compared it to the invention of writing and the art of printing ; moreover, he has placed it on an equality with the Galvanoplastics of Jacobi and Spencer, and the Daguerreotype of Daguerre. Valuable as are the results of Nature-Printing, it still has its defects ; it has its limits, and its applications are limited, and care will be required to confine it within the bounds of its capabilities.

That an establishment, so renowned for its productions as that at Vienna, unlimited in its command of the resources of science and mechanism, should have been the first to bring any invention connected with printing to a practical state of perfection, is not matter to create surprise ; but that it should, in the most unqualified manner, in the name and on the authority of its chief director, claim all the honour of the discovery, is a point that is open to question, and in point of fact is questioned by several private individuals, who, for want of those unlimited resources and opportunities which only government establishments are able to command, were unable to crown their experiments with practical results.

Nature-Printing is nothing more than an application of facts worked out by various persons, in different countries, under very different circumstances, and at very different periods ; and by tracing

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* *Vide* Denkschriften der Kais. Akademie, Wien ; Math.-Nat. Classe. *Band V., p. 107 (illustrated by many plates).*

out its history, and detailing the earlier experiments connected with it, Mr. Bradbury hoped to show that he did not put forward personally any claim either to its origin or to its first application; but, that he spoke as one who, having perceived its value, was desirous to render it an available auxiliary to the printing-press.

Nature herself, in her mysterious operations, seems to have given the first hint upon the subject: witness the impressions of Ferns so beautifully and accurately to be seen in the coal-formations.

Experiments to print direct from nature were made as far back as about two hundred and fifty years—it is certain that the present success of the art is mainly attributable to the general advance of science, and the perfection to which it has been brought in particular instances.

On account of the great expense attending the production of woodcuts of plants in early times, many naturalists suggested the possibility of making direct use of nature herself as a copyist. In the *Book of Art*, of Alexis Pedemontanus, (printed in the year 1572,) and translated into German by Wecker, may be found the *first* recorded hint as to taking impressions of plants.

At a later period—in the *Journal des Voyages*, by M. de Moncoys, in 1650, it is mentioned that one Welkenstein, a Dane, gave instruction in making impressions of plants.

The process adopted to produce impressions of plants at this period, consisted in laying out flat and drying* the plants. By holding them over the smoke of a candle, or an oil lamp, they became blackened in an equal manner all over; and by being placed between two soft leaves of paper, and by being rubbed down with a smoothing-bone, the soot was imparted to the paper, and the impression of the veins and fibres was so transferred.

Linnæus, in his *Philosophia Botanica*, relates that in America, in 1707, one Hessel made impressions of plants; and between 1728 and 1757, Professor Kniphof, at Erfurt, who refers to the experiments of Hessel, in conjunction with the bookseller Fünke, established a printing-office for the purpose. He produced a work entitled *Herbarium Vivum* [a copy of which was laid before the Members]. The range and extent of his work, twelve folio volumes, and containing 1200 plates, corroborates the curious fact of a printing-office being required. These impressions were obtained in a manner very similar, but with the substitution of printer's ink for lamp-black, and flat pressure for the smoothing-bone. A new feature at this time was introduced—that of colouring the impressions by hand, according to Nature—a proceeding which though certainly con-

* Although the plants were dried in every case, Mr. Bradbury stated, that it was by no means absolutely necessary, as he proved by the simple experiment of applying lamp-black or printer's ink to a fresh leaf, and producing a successful impression.

tributing to the beauty and fidelity of the effect, yet had the disadvantage of frequently rendering indistinct, and even sometimes totally obliterating, the tender structure and finer veins and fibres. Many persons at the time objected to the indistinctness of such representations and the absence of the parts of fructification; but it was the decided opinion of Linnæus, that to obtain a fac-simile of the difference of species was sufficient.

Seligmann, an engraver at Nuremberg in 1748, published in folio plates figures of several leaves he had reduced to skeletons. As he thought it impossible to make drawings sufficiently correct, he took impressions from the leaves in red ink, but no mention is made of the means he adopted. Of the greater part he gave two figures, one of the upper and another of the lower side.

Even at this early period the idea must have excited much attention; for it is recorded that Seligmann had announced his intention to give figures of natural objects as magnified by a solar microscope, and that two were to have been published every month. But he died soon after, and a law-suit prevented the prosecution of his work. Two black and twenty-nine red plates of leaves had been already completed, and were published with eight pages of text, in which his coadjutor, Crew, speaks of the physiology of plants, and Seligmann of the preparation of leaf skeletons. The leaves represented on the plates were those of the orange, lemon, shaddock, &c.

In the year 1763 the process is again referred to in the *Gazette Salutaire*, in a short article upon a *Recette pour copier toutes sortes de plantes sur papier*.

About from twenty-five to thirty years later, Hoppe edited his *Ectypa Plantarum Ratisbonensium*, and also his *Ectypa Plantarum Selectarum*, the illustrations in which were produced in a manner similar to that employed by Kniphof. These impressions were found also to be durable, but still were defective. The production of impressions could only take place very slowly, as the blacking of the plants with the printer's ball required much time. Rude as the process was, and imperfect the result, it was nevertheless found that the figures thus produced were far more characteristic than any which artists could produce. The fault of the method consisted in its limited application and in its incompleteness; since the fragile nature of the prepared plant, if ever so carefully treated, would admit of but very few copies being taken, and where any great number would have been required, many plants must have been prepared, a circumstance which was in itself a great obstacle.

In the year 1809 mention is made in Pritzell's "Thesaurus" of a *New Method of taking Natural Impressions of Plants;* and lastly, in reference to the earlier history of the subject, the attention of scientific men was called to an article, in a work published by Grazer, in 1814, on a *New Impression of Plants.*

Twenty years afterwards, the subject had undergone remarkable change, not only in the mode of operation to be pursued, but also

in the result produced,—which consisted in fixing an impression of the prepared plant in a plate of metal by pressure.

It appears, on the authority of Professor Thiele, that Peter Kyhl, a Danish goldsmith and engraver, established at Copenhagen, applied himself for a length of time to the ornamentation of articles in silver ware, and the means he adopted were, taking copies of flat objects of nature and art in plates of metal by means of two steel rollers.

Various productions in silver of this process were exposed in the Exhibition of Industry held at Charlottenburgh, in May 1833. In a manuscript, written by this Danish goldsmith, entitled *The Description (with forty-six plates) of the Method to Copy Flat Objects of Nature and Art*, dated 1st May, 1833, is suggested the idea of applying this invention to the advancement of science in general. The plates accompanying this description represented printed copies of leaves, of linen and woven stuffs, of laces, of feathers of birds, scales of fishes, and even of serpent-skins.

The manuscript contains ample and clear instructions to carry out the method, and a few extracts, in his own words, of the leading features will be perhaps interesting. He thus writes:—

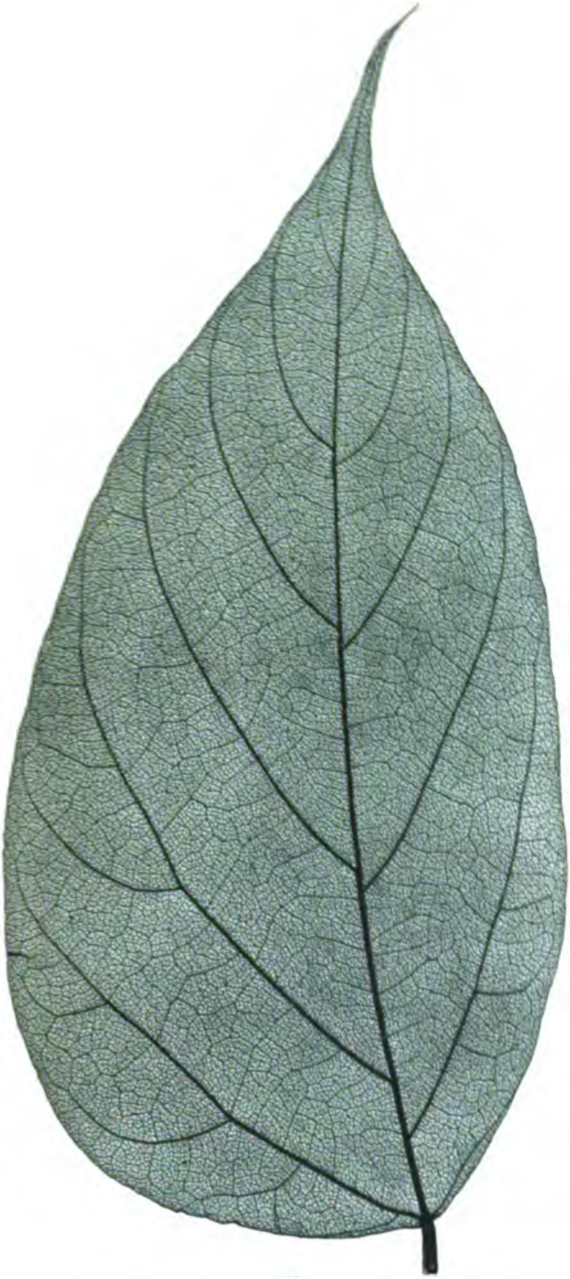
"As a correct copy of the productions of Nature and Art must be of great importance, I am delighted to have the honour of submitting to the friends of Art and Science a method I have discovered, by which copies of most objects can be taken, impressed into metal plates, and which enables the naturalist and botanist to get representations of leaves, feathers, scales, &c., in a quick and easy way; and these copies will give all the natural lineaments, with their most raised or sunken veins and fibres; moreover, the artist can, by means of this invention, make use of Nature's real peculiarities for ornamental compositions and productions; and the merchant can get patterns of delicately woven or figured stuffs, laces, tickens, ribbon, linen, and so forth.

"To fix an impression into a plate of copper, zinc, tin, or lead, properly prepared for the purpose, a rolling machine with two polished cylinders of steel is required; if a *leaf*, quite dried and prepared, is placed between a polished steel plate half an inch thick and a thoroughly heated lead plate with a fine surface, and these two plates with the leaf between be run speedily between the cylinders, the leaf will by the pressure yield its form on the softer lead plate, precisely as it is shaped, with all its natural raised and sunken parts.

"I tried many ways to fix the leaf on the plate by some glutinous matter, but it filled the delicate pores and deep parts so much as to render the copies very indistinct.*

"The printing itself of the leaf into the metal requires much

* Mr. Bradbury stated that he had himself tried this method without success.



NATURE PRINTING.

precaution, especially with respect to placing the cylinders exactly parallel, and at the same time at a proper distance, and to have the plate to be stamped carefully burnished and polished; besides, the utmost care must be used, as particles of dust or dirt would be printed together with the object itself. Moreover, care must be taken, that the rolling of the plates is managed well, so as to run parallel, without deviating from their first direction.

"Leaves that are to be printed must first be spread upon a clean sheet of paper and placed upon a warm oven; a second sheet put over them is to be strewn with sand, and the whole left to dry under a weight. This done the leaves are taken out with due precaution, and placed for a quarter of an hour into water. They are dried again in the same way, and this manipulation is repeated four or five times. By this means I always found that the leaves gained in tenacity and firmness, that they lost all their moisture, and became more fit to be stamped. Objects, such as laces, weavings, figured ribbons, and such like, can be printed without any preparation, provided they be spread flat between the plates.

"The season being very unfavourable for gathering good strong leaves, I had to overcome many difficulties, so that the copies are not so good as they might have been—for I have observed that leaves obtained from green-houses do not yield such distinct prints as those that grow in the open air, when properly developed."*

It would appear from the practical hints here given that Peter Kyhl was no novice at the process. He distinctly points out what he conceives to be its value, by the subjects that he tried to copy; and he enters into detail on the precautions to be observed in the operation of impressing metal plates so as to ensure successful impressions. His manuscript explains that he had experimented with copper, zinc, tin, and lead plates. Still there existed obstacles which prevented him from making a practical application of his invention. In the case of zinc, tin, and copper, the plant, from the extreme hardness of the metals, was too much distorted and crushed; while in lead, though the impression was as perfect as could be, there was no means of printing many copies, as it was not possible after the application of printer's ink to retain the polished surface that had been imparted to the lead plate, or to cleanse it so thoroughly as to allow the printer to take impressions free from dirty stains. This was a serious obstacle, which was not compensated for even by the peculiar rich surface of the parts that were impressed, attributable to the lead being more granular than copper, and which is

* This allusion to the want of tenacity and firmness in young, and especially in green-house plants, is quite consistent with the experiment made at the present time. Mr. Bradbury stated, that to obtain an impression at all, upon a plate of metal, of a plant, it was indispensable that the plant should be thoroughly dried and free from sap; otherwise the plant would spread in all directions, without leaving any visible indentation. Objects such as lace, and figured fabrics, can be impressed without any preparation, provided they be spread flat between the plates.

so favourable to adding density or body of colour, without obliterating the tender veins and fibres. Peter Kyhl died in the same year that he made known his invention. At his death, his manuscripts and drawings were deposited in the archives of the Imperial Academy of Copenhagen, where they remained for upwards of twenty years: and it is a remarkable fact, that, shortly after his death, was discovered the only thing wanting to render the process, as explained by him, at once available for practical purposes. Had Kyhl lived to prosecute his experiments, he might have accomplished more than he did without requiring the aid of other means.* It was he who discovered how to take impressions in metal plates, by using steel-rollers.

This is the first element in the process of Nature-Printing. It fell to Dr. Ferguson Branson, of Sheffield, to suggest the second, and the most important.

During the next twenty years Nature-Printing was but indifferently prosecuted by various persons for various purposes. Mr. Taylor, of Nottingham, as far back as 1842, printed lace, &c., specimens of which were exhibited at the Great Exhibition; and Mr. Twining, of Nottingham, in 1847, printed ferns, grasses, and plants, which were exhibited by the Botanical Society of London. He adopted the same plans as those used by Kniphof and Hoppe.

In 1847, also, Dr. Ferguson Branson commenced a series of experiments, an interesting paper upon which was read before the Society of Arts in 1851, and therein for the first time was suggested the application of that second and most important element in Nature-Printing which is now its essential feature—the application of the Electrotype.

"I beg leave," says he, "to bring before the notice of the Society of Arts a new method of engraving plates for printing ferns, leaves, seaweeds, and other flat plants. . . . . Having taken in gutta-percha some impressions of ferns, the singularly beautiful manner in which the exact character of the plant was transferred to the gum suggested to me the possibility of printing from the gutta-percha itself, so as to produce on paper a *fac-simile* of the plant. That experiment partially succeeded, and curiously tested the elasticity of the substance; for the impression remained uninjured, after being subjected to the great pressure of a copper-plate roller. I say that it *partially* succeeded; for the printer found it utterly impossible so thoroughly to cleanse the ink from the margin around the impression, as not when printed to leave a dirty stain on the paper. The impressions thus produced were very accurate; but the process was valueless as regards multiplication of the prints."

It then occurred to Dr. Branson that an electrotype copy would obviate the difficulty.

* Kyhl, as it was, had had his attention directed, and had made experiments to overcome this one remaining difficulty. His manuscript also contains many interesting and practical remarks upon other processes than simply Nature-Printing.

He afterwards stated that he abandoned the process of electrotyping in consequence of his finding it tedious, troublesome, and costly, to produce large plates. Having occasion, however, to get an article cast in brass, he was astonished at the beautiful manner in which the form of the model was reproduced in the metal. He determined, therefore, to have a cast taken in brass from a gutta-percha mould of ferns, and was much gratified to see the impression rendered almost as minutely as by the electrotype process;* but, however curious his individual specimens, the process produced no practical result.

In 1849, Professor Leydolt, of the Imperial Polytechnic Institute at Vienna, availed himself of the resources of the Imperial Printing-Office to carry into execution a new method he had conceived of representing agates and other quartzoze minerals in a manner true to nature. Professor Leydolt had occupied himself for a considerable period in examining the origin and composition of these interesting objects in geology. In the course of his experiments and investigations he had occasion to expose them to the action of fluoric acid, when he found, in the case of an agate, that many of the concentric scales were totally unchanged, while others, to a great extent, decomposed by the acid, appeared as hollows between the unaltered scales. It occurred to Leydolt that the surfaces of bodies thus corroded might be printed from, and copies multiplied with the greatest facility.

The simplest mode for obtaining printed copies is to take an impression direct from the stone itself. The surface after having been etched is well washed with dilute hydrochloric acid and dried; then carefully blackened with printer's ink. By placing a leaf of paper† upon it, and by pressing it down upon every portion of the etched or corroded surface with a burnisher, an impression is obtained, representing the crystallised rhomboidal quartz *black*, and the weaker parts that have been decomposed by the action of the acid *white*. It requires but a small quantity of ink—and particular care must be exercised in the rubbing down of the impression. This mode is good as far as it goes—but it is slow and uncertain—and incurring a certain amount of risk, owing to the brittle nature of the object; and the effect produced is not altogether correct, since it represents those portions black that should be white, and those white that should be black.

The stone is not sufficiently strong to be subjected to the action of a printing-press; an exact *fac-simile* cast, therefore, of it must be obtained, and in such a form as can be printed from. To effect this, the surface of any such stone (previously etched by corrosion) must be extended by imbedding it in any plastic composition that

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* The casting in brass is a very interesting experiment—but its results cannot be compared with the production of the electrotype.

† *India-paper* and *Chalk-paper* are the best adapted for the purpose.

will yield a perfectly flat and smooth surface, so that the surrounding surface of the plastic composition will be exactly level with the surface of the etched stone: all that is necessary now is to prepare the electrotype apparatus, by which a perfect *fac-simile* is produced, representing the agate impressed, as it were, into a polished plate of copper. This forms the printing-plate. The ink in this case, as opposed to the mode before referred to, is not applied upon the surface, but in the depressions caused by the action of the acid on the weaker parts; the paper is forced into these depressions in the operation of printing, which results in producing an impression in relief—a feature that is rather peculiar to the process, as the raised appearance, especially in the case of plants, adds very much to their effect.

The impressions printed in this latter manner present far more beautiful and natural representations, since the crystallised quartz are represented *white*, while the decomposed parts appear *black*.

Professor Leydolt, however, suggests that some corroded stones are better suited sometimes for one method of representation than the other; and attention should be paid to this while the stones are being exposed to the action of the acid. He considers that important advantages will result to science from the perfect faithfulness of such representations, and from the facility and inconsiderable expense of their production.

Other objects in geology—such as the fossil remains of fishes, plants, and other organic remains—in some cases can be, and have been, copied with unmistakeable resemblance to the original.

It is not clear who may have suggested the possibility of creating impressions of these last-named objects, but one thing is beyond a doubt, that the production of them was left entirely to the judgment of Andrew Worring, as was also the case in the production of the agates and other stones.

In operating upon this class of objects, it is desirable that the original should be as flat as possible, as the flatter the general surface is, the more successful will be the effect produced.

A mould in the first place is taken with gelatine or liquid gutta-percha, the elasticity of which materials are favourable for flattening the mould without distortion when separated from the original,—a mode that is to be preferred to depositing copper direct upon them, since it is very much more easily manipulated and without the slightest risk of damaging the originals, owing to the absence of pressure.

This gelatine or gutta-percha mould is rendered metallic or conducting in the usual way by the application of plumbago, and copper is deposited until of sufficient thickness to form a printing plate.

In 1852, Mr. Aitken, of Birmingham, followed the footsteps of Kyhl in various experiments made by him in Britannia metal. He took impressions of lace, skeleton-leaves, feathers, &c., in Britannia metal, for the purpose of ornamentation, in the same way as Kyhl is said to have done in articles of silver. About this

period Dr. Branson again made experiments, and endeavoured to bring Nature-Printing into practical operation. He too tried impressions on Britannia metal, not altogether with the view of printing direct from such plates, desirable as it would be to dispense with the operation of taking casts—but of transferring impressions to stone; and after printing an impression in some neutral tint, to resort to colouring by hand. (Specimens of this method were lying on the table; but, on examination, would not bear comparison with the productions of the present time.)

In the Imperial Printing-office of Vienna, the first application of taking impressions of lace on plates of metal, by means of rollers, took place in the month of May, 1852:* it originated in the Minister of the Interior, Baumgartner, having received specimens from London, which so much attracted the attention of the chief Director, that he determined to produce others like them. This led to their using gutta-percha in the same way that Dr. Branson had used it; but finding this material did not possess altogether the necessary properties, the experience of Andrew Worring induced him to substitute lead, which was attended with remarkable success. Professor Haidinger, on seeing specimens of these laces, and learning the means by which they had been obtained, proposed the application of the process to plants. The results of these experiments,† as well as those of Professor Leydolt above referred to, appeared in the fifth volume of Memoirs of the Imperial Academy, published at Vienna, in 1850.

Up to this time, however, in England, notwithstanding the above-mentioned experiments, the discovery had not assumed any practical form; but there is little doubt that if any of these persons had had the requisite means and appliances it would have been brought to perfection earlier. These consist mainly in the precipitation of metals upon moulds or matrixes by means of electro-galvanic agency.

Nature-Printing owes its present success to the electrotype, which was then, and even at the present time is, the only means by which *faithful* copies can be taken of those delicate fibrous details that are furnished in the examples of the impressions of botanical and other figures in metal. It may be said to be owing to the extensive scale upon which the process of the electrotype is conducted in the Imperial establishment, that Worring was enabled to render the process of Nature-Printing practically available as a Printing Art.

The deposition of metals by galvanic agency, though long known and practised in England, has been considered more as a *scientific* than a *practical* mode of casting; and it is only within the last few years that its value in its manufacturing capabilities has been

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* The Austrian patent was taken out on the 12th October, 1853, in the name of Andrew Worring.

† These consisted of specimens of lace, leaves, plants, mosses, serpent-skins, the wing of a bat, agates, fossils, and petrifactions; and it is somewhat curious that these examples were similar in character to those chosen by Kyhl.

properly understood. Up to within a short time it has been found uncertain, difficult, tedious, expensive, and requiring great length of time to obtain adequate results from it; but Mr. Bradbury stated that he had for the last two years devoted his energies to overcome these difficulties, and that his experiments had been attended with many practical advantages in the Art of Printing. On the table before him he had a small electrotype apparatus, by which was produced a perfect electrotype cast of an impressed metal plate before the audience in half an hour.* He stated, that one of his experiments had been crowned with such success that he had reduced the operation of the battery and the decomposition trough to so rapid and certain a result as to be able to duplicate the woodcuts contained in a number of the *Illustrated London News*, no matter what their number or size, in the short space of twelve hours (ready in every respect for the press), which he stated as his belief was one of the greatest practical accomplishments that had ever been made in any country in this branch of science; the value of which to the journal in question will be best understood when it is known, that without *this* or other means (not yet discovered), the production of the requisite number of copies in time for publication would be a mechanical impossibility (so extensive is its circulation) since from *one* set of engravings there is a limit to the number of impressions that can be printed from *one* machine in a given time.

The mode of printing these electrotype † plates of plants is the same as in ordinary copper-plate printing, where the impression is produced by passing the inked plate with the sheet of paper laid upon it through a pair of rollers, one of which is covered with four or five thicknesses of blanketing, which causes the peculiar raised or embossed appearance of the impression.

In such cases, where there are three, four, or more colours, for instance,—as in flowering plants, having stems, roots, leaves, and flowers,—the plan adopted in the inking of the plate is to apply the darkest colour first, which generally happens to be the roots—the superfluous colour is cleaned off,—the next darkest colour, such as perhaps the colour of the stems, is then applied—the superfluous colour of which is also cleared off,—this mode is continued until every part of the plant in the copperplate has received its right colour. In this state, before the plate is printed, the colours in the different parts of the copper look as if the plant was imbedded in copper. By putting the darkest colour in at the beginning, there is less chance of smearing the lighter ones: the printer too is not only

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* In the afterpart of the evening Mr. Bradbury succeeded in producing thin electro-plates of impressed plates in five minutes.

† The copper deposited upon moulds by electro-galvanic agency, is precipitated in such inconceivably small atoms, that the defects previously referred to in the surface of the lead plate, are also *faithfully* copied, but the surface of copper (unlike that of lead) will allow of these defects being removed by the aid of the burnisher, and a polished surface preserved.

able by this means to blend one colour into another, but to print all the colours at one single impression.

The *first* practical application of Nature-Printing for illustrating a botanical work, and which has been attended with considerable success, is Chevalier Von Heufler's work on the Mosses,* collected from the Valley of Arpasch, in Transylvania; the *second*, (the *first in this country*,) is the "Ferns of Great Britain and Ireland," in course of publication, under the editorship of Dr. Lindley, and printed by Messrs. Bradbury and Evans. Ferns, by their peculiar structure and general flatness, are especially adapted to develope the capabilities of the process, and there is no race of plants where minute accuracy in delineation is of more vital importance than the Ferns; in the distinction of which, the form of indentations, general outline, the exact manner in which repeated subdivision is effected, and most especially the distribution of veins scarcely visible to the naked eye, play the most important part. To express such facts with the necessary accuracy, the art of a Talbot or a Daguerre would have been insufficient until Nature-Printing was brought to its present state of perfection.

Mr. Bradbury then adverted to the ingenious and beautiful productions of Felix Abate, of Naples. His Nature-representations consist of sections of wood, in which the grain is admirably represented. He terms his peculiar process Thermography, or the Art of Printing by Heat. The process consists in wetting slightly the surface of the wood of which *fac-similes* are to be made, with any diluted acid or alkali, and then taking an impression upon paper, or calico, or white wood; the impression is quite invisible, but by exposing it for a few instants to a strong heat, the impression appears in a more or less deep tone, according to the strength of the acid or alkali. In this way every gradation of brown from maple to walnut is produced; but for some woods which have a peculiar colour, the paper, &c. is to be coloured, either before or after the impression, according to the lightest shades of the wood. Abate, in his manipulations, also employs the ordinary dyeing process.

It is to be hoped that Abate's process may become alike useful to the natural sciences and the decorative arts.

Mr. Bradbury stated, in conclusion, that we are indebted to—

Kniphof, for the application of the process in its rude state;

Kyhl, for having first made use of steel-rollers;

Branson, for the suggestion of the electrotype;

Leydolt for the remarkable results he obtained in the representation of flat objects of mineralogy, such as agates, fossils, and petrifactions;

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* *Specimen Floræ Cryptogamæ vallis Arpasch Carpatæ Transylvani*; Conscripsit Ludovicus Eques de Heufler. Vienna, 1853. Imp. folio.

Haidinger, for having promptly suggested the impression of a plant into a plate of metal at the very time the *modus operandi* had been provided;

Abate, for its application to the representation of different sorts of ornamental woods on woven fabrics, paper, and plain wood;

Worring, of the Imperial Printing-Office, Vienna,* for his practical services in carrying out the plans of Leydolt and Haidinger.

Nature-Printing may be considered as still in its infancy; but the results, already obtained in its application, encourage us to expect from continued efforts such further improvements as will place it not least among the Printing Arts.

[H. B.]

[A great number of specimens of Nature-Printing, in its various applications, were exhibited; and the different processes referred to by the speaker, were exemplified in the presence of the audience, during and after the discourse, by workmen and apparatus from the establishment of Messrs. Bradbury and Evans.]

WEEKLY EVENING MEETING,

Friday, May 18.

Rev. John Barlow, M.A. F.R.S. Vice-President and Secretary, in the Chair.

James Philip Lacaita, Esq. LL.D.

On Dante and the "Divina Commedia."

The speaker, after a few preliminary remarks, proceeded to state, that he should not attempt to give an account of the life of Dante, which was so connected with the chief events of his time, that it was impossible to sketch it with any degree of interest, without entering into many details of the mediæval history of Italy. Carlo Troya, and Count Cesare Balbo, two of the most profound Italian

* It is gratifying to know that the services of this gentleman were recognised by his Sovereign, who munificently rewarded him with a gift, and likewise the Order of Merit.

NATURE PRINTING.

historians of this century, whose recent loss their countrymen have so much reason to regret, might be adduced as illustrations of the statement. Troya, by his researches on Dante's life, and on the meaning of the well-known lines—

> " . . . . Infin che 'l Veltro
> Verrà, che la farà morir di doglia."
> *Inf.* i. 101–102.

was led to write a mediæval history of Italy; and Balbo, by a converse process, ended his studies on the mediæval history of Italy, by writing a life of Dante.

There was an event in that life, however, which he would not omit to notice, as it had a peculiar interest for an English audience. Dante visited, and most probably attended a course of theology at, Oxford. Boccaccio asserts, in some Latin verses, which he addressed to Petrarca, in sending him a copy of the "*Commedia*," that Dante had been

> ". . . . Parisios dudum, extremosque Britannos."

Boccaccio, who was born in 1313, had certainly heard it from his father, who resided in Paris as a merchant; and who, being a Florentine, had no doubt known, and perhaps been familiar with, Dante. John, of Serravalle, Bishop of Fermo, in 1416, translated into Latin, and expounded the "*Commedia*," at the request of Cardinal Amadeo de Saluces, and of the Bishops of Bath and Salisbury, whom he had met at the Council of Constance. In the preface to his translation, which is in MS. in the Vatican library, Serravalle says: "*Dantes dilexit Theologiam sacram, in qua diu studuit tam in Oxoniis in regno Angliae quam Parisiis;*" and again: "*Se in juventute dedit omnibus artibus liberalibus, studens eas Paduae, Bononiae, demum Oxoniis et Parisiis.*" The lines allusive to the murder of the nephew of Henry III., in the church of Viterbo, by Guy de Montfort:—

> "Mostrocci un' ombra dall' un canto sola,
> Dicendo, colui fesse in grembo a Dio
> Lo cor che 'n sul Tamigi ancor si cola."
> *Inf.* xii. 118-120.

also evidence the same fact; for they convey an impression that Dante had himself seen the place in which the head of the murdered youth was preserved. His visit to Oxford must have been between 1308 and 1311, when, after leaving the Malaspinas, he went to Paris.

The speaker expressed a wish that some one of the sons of that great seat of learning would enquire fully into the subject, to which as yet no attention had been paid, and to the glories of his "Alma Mater," add that of having received within her walls the greatest poet of Christendom.

A rapid enumeration was then given of the minor works of Dante:—the *Vita Nuova;* the *Convito;* the *Poesie Minori;* the treatise *De Monarchia;* the treatise *De Vulgari Eloquio;* and several Latin letters. With regard especially to his minor poems, it was observed that with the exception of a few sonnets, and the ode to Florence, they are modelled on the Provençal School, and are a mixture of conventional poetry and scholastic theology, which would scarcely be recognised as proceeding from the same author as the *Commedia.*

The speaker proceeded next to the great poem, which was called by Dante *La Commedia*, a name it preserved in many of the earlier editions till the end of the fifteenth century, when the epithet of "*Divina*" was added to it. He gave a short account of the different opinions with regard to what may have suggested the *idea* of the poem, and noticed how Fontanini supposed it had been suggested by a novel of the day containing a description of St. Patrick's well; while Denina would assign a like honour to two French novels of the 12th century; Ginguené to the *Tesoretto* of Brunetto Latini; Villemain to a sermon of Gregory VII., containing the account of a vision of the other world; and Cancellieri and others to the *Visione di Frate Alberico*, whose original manuscript is still preserved in the library of the Benedictine Monastery of Monte Casino. He concluded by saying, that the multiplicity of the sources from which it was maintained to have been derived, went only to prove, not that Dante had borrowed the idea from any previous composition, but that the *vision* was a prevailing form of the literature of the time; a form which might be said to have been chiefly introduced and made popular by the spurious Gospels of the second century, pretending to give an account of St. Paul's ascent to the third heaven, and by the *Pastor* of Hermas. It was worthy of remark, that Dante, when only nine years old, on seeing and admiring Beatrice, one year younger than himself, wrote a sonnet, which caused him to be favourably noticed by the contemporary poets, except Dante da Maiano, who ridiculed him; and that the form that his thoughts assume, even at that very early age, is that of a *vision* or a *dream.*

After alluding to the various controversies which for five centuries had been raised concerning the *allegory* of the poem, the speaker stated that he adopted Troya and Balbo's historical explanations of most of the allegorical passages. He pointed out the absurdity of the hidden anti-papal spirit supposed to run through the whole poem; a theory first hinted by Ugo Foscolo, and afterwards enlarged upon by Gabriele Rossetti. He conceived that Dante was strictly orthodox in his Roman Catholic tenets; and he felt no hesitation in asserting that a learned theological reader might almost consider the *Commedia*, especially the "*Paradiso*," as a poetical *synopsis* of the *Summa Theologica* of St. Thomas Aquinas, whose leading tenets were propounded throughout the poem, clothed in

the most beautiful poetical language, such as Dante alone had the power of combining with the scholastic theology. It was the temporal power of the Popes that Dante so constantly attacked, and that in no hidden way, as might be seen by a reference to three beautiful passages in *Inf.* xix. 46–123, *Purg.* xvi. 97–132, and *Par.* xxvii. 1–66. Dante in this respect might be considered a proof that the teaching of Arnaldo da Brescia had taken hold on the Italian minds. Mr. Lacaita then briefly commented on the controversy raised with regard to the orthodoxy of Dante at the time of the Reformation, and the strange decision given by the Père Hardouin, that *La Commedia* was the work of a disciple of Wicliff.

He afterwards took a rapid survey of the fluctuations of the estimation in which the *Commedia* had been held at different times; as a proof of which he noticed that the poem, from 1420 to 1500, had gone through 20 editions; through 42, from 1501 to 1597; through 4, from 1598 to 1727; through 42, from 1728 to 1800; and through more than 180, from 1800 to 1850. He ascribed the neglect into which it had fallen during the whole of the 16th century to the influence of Spanish rule, and the power of the Inquisition in Italy; and pointed out how the falling off of taste in literature, and even in the Fine Arts in Italy had been, if not consequent upon, at least simultaneous with, such neglect. The poem was well known in England in the 14th and 15th centuries; passages were quoted in which Chaucer had alluded to, or translated from it. But afterwards the poem seems to have been nearly forgotten, till attention was again called to it by a first English translation in 1773. A few observations were here introduced on the respective merits of the various English translations; and Mr. Pollock's recent translation was particularly noticed for its faithful conveyance of the meaning of the original. The speaker afterwards proceeded to say, that it was remarkable that Addison seems to have ignored, if not the existence, at least the great merits of the *Commedia*, so far, that in his journey to Italy, although he describes several monuments at Ravenna, he does not even allude to the tomb of Dante, which only a few years before his visit had been restored by Cardinal Corsi. It was a curious coincidence that at the revival of the study of Dante in Italy, in the 18th century, Voltaire and the ex-jesuit Bettinelli both agreed, though from different motives, in attacking Dante; Bettinelli, in his *Lettere Virgiliane*, went even further than Voltaire, who admitted that the *Commedia* was, "Un ouvrage bizarre; mais brillant de beautés naturelles, ou l'auteur s'élève dans les détails au dessus du mauvais goût de son siècle et de son sujet."

After warmly contending against the preference sometimes given of the *Inferno* as the finest part of the poem, a preference explained perhaps by the fact that many never read the *Purgatorio* and the *Paradiso*, which *nevertheless* display, when compared with the In-

ferno, finer poetical expression, finer powers of description, more gentle and nobler feelings, and a total freedom from coarseness of allusion: the speaker went on to censure F. Schlegel's assertion that the chief defect of the poetry of Dante is *a want of gentle feelings;* he felt sure that the German critic had scarcely glanced at the *Purgatorio* and the *Paradiso*. He proceeded next to point out what he conceived to be the finest passages in the *Purgatorio*, which from the 1st to the end of the 31st canto is an almost uninterrupted flow of soft and brilliant poetry. He called particular attention to the beautiful opening of the 1st canto; to the touching meeting of Casella, ii. 67–133; the meeting of Manfredi, iii.; Buonconte di Montefeltro and La Pia de' Tolommei, v. 88–136; the meeting of Virgil with Sordello, and the splendid apostrophe to Italy and to Florence, vi. 58–151; and to the whole 8th canto, one of the finest in the poem. In quoting the beautiful lines in praise of the Malaspinas, the speaker mentioned that 520 years after Dante had found hospitality among them, another exile, Carlo Troya, driven away from Naples when Austrian bayonets had suppressed the Neapolitan constitution in 1821, was also hospitably received by a Malaspina, with whom he went wandering through Val di Magra, and collecting the local traditions connected with the residence of Dante in that mountainous district. Mr. Lacaita further referred the audience to the description of sculptures, the story of Trajan and the poor woman, x. 28–96, 121–129; Oderisi d'Agubbio, Cimabue, Giotto, and the beautiful lines on worldly fame, xi. 73–117; Sapia from Siena, xiii. 91–154; Guido del Duca, the Val d'Arno, and the Romagna, xiv. 16-126; the speech of Marco Lombardo, and the allusion to the temporal power of the Popes, xvi. 67–129; Pope Adrian V., xix. 100–145; Ugo Capeto and Pope Boniface VIII., xx. 43–96; Forese de' Donati's praises of his widow, and censure of the Florentine ladies, xxiii. 76–111; Forese's mention of his sister Piccarda, Buonagiunta da Lucca, Dante's poetry, &c., xxiv. 1-90 and 145-154; Guido Guinicelli, xxvi. 91-135; Dante's dream, &c. xxvii. 70-142; the terrestrial Paradise, and meeting of Matelda, xxviii. 1-63; the meeting of Beatrice, the parting of Virgil, and reprimand of Beatrice to Dante, xxx. 22-145; and the whole canto, xxxi.

The speaker regretted that time did not allow him to point out in the same way what he considered to be the finest passages in the *Paradiso*. After some general remarks on the peculiar character and suggestiveness of Dante's poetry, on the truth and wonderful variety of his similes, on the essentially moral tendency of the whole poem, &c., he concluded by quoting the following passage from a very able essay on Dante by Mr. Church, in the *Christian Remembrancer*, which embodied, better than he could express by words, his feelings in parting with a subject, to which he felt he could scarcely have done adequate justice.

"Those who know the *Divina Commedia* best, will best know

how hard it is to be the interpreter of such a mind; but they will sympathise with the wish to call attention to it. They know, and would wish others also to know, not by hearsay, but by experience, the power of that wonderful poem. They know its austere, yet subduing beauty; they know what force there is in its free and earnest and solemn verse, to strengthen, to tranquillize, to console. . . . But, besides this, they know how often its seriousness has put to shame their trifling, its magnanimity their faintheartedness, its living energy their indolence, its stern and sad grandeur rebuked low thoughts, its thrilling tenderness overcome sullenness and assuaged distress, its strong faith quelled despair and soothed perplexity, its vast grasp imparted the sense of harmony to the view of clashing truths. They know how often they have found, in times of trouble, if not light, at least that deep sense of reality, permanent, though unseen, which is more than light can always give—in the view which it has suggested to them of the judgments and the love of God!"

[J. P. L.]

WEEKLY EVENING MEETING,

Friday, May 25.

Sir Charles Fellows, Vice-President, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On Electric Conduction.

Since the time when the law of definite electrolytic action was first laid down (*Exp. Res.* 783-966), it has become a question whether those bodies which form the class of electrolytes, conduct only whilst they are undergoing their proper change under the action of the electric current; or whether they can conduct also as metals, dry wood, spermaceti, &c. do in different degrees, *i. e.* without the accompaniment of any chemical change within them. The first kind of conduction is distinguished as the *electrolytic;* the transference of the electric force appearing to be essentially associated with the chemical changes which occur: the second kind may be called *conduction proper;* and there the act of conduction leaves the body ultimately as it found it. Electrolytic conduction is closely associated with the liquid state, and with the compound

nature and chemical proportions of the bodies in which it occurs; and it is considered as varying in degree (*i. e.* in facility) with the affinities of the constituents belonging to these bodies; there are, however, other circumstances which evidently, and indeed very strongly affect the readiness of transfer, such as temperature, the presence of extraneous matters, &c. Conduction proper differs as to facility by degrees so far apart, that that quantity of electricity which could pass through a hundred miles of one substance, as copper, in an inappreciably small portion of time, would require ages to be transmitted through the like length of another substance, as shell-lac; and yet the copper with its similars offers resistance to conduction; and the lac, and its congeners, conduct.

The progress and necessities of science have rendered it important within the last three or four years, and especially at the present moment, that the question "whether an electrolyte has any degree of conduction proper" should be closely considered, and the experiments which are fitted to probe the question have been carried to a very high degree of refinement. Buff,* by employing the electric machine, and Wollaston terminals, *i. e.* platinum wires sealed into glass tubes, and having the ends only exposed, has decomposed water by a quantity of electricity so small that it required four hours to collect gas enough to fill a little cylinder only $\frac{1}{10}$th of an inch in diameter, and the $\frac{1}{5}$th of an inch in length; yet the decomposition was electrolytic and polar; and therefore the conduction was electrolytic also. When one pole only was in the water, and the other in the air over it, still the decomposition, and therefore the conduction, was electrolytic; for one element appeared at the pole in the water, and the other in the air or gas over the water at the corresponding pole. Buff concludes that electrolytes have no conduction proper. Many other philosophers have supported, with more or less conviction, the same view, and believe that electrolytic conduction extends to, and includes cases, which formerly were supposed to depend upon conduction proper. Soret advances certain experimental results,† but reserves his opinion from being absolute. Von Breda and Logeman adopt the more general view unreservedly.‡ De la Rive, I think, admits that a very little may perhaps pass by conduction proper, but that electrolytic conduction is the function of electrolytes.§ Matteucci has at one time admitted a little conduction proper, but at present, I believe, denies that any degree exists. On the other hand, Despretz,‖ Leon Foucault,¶ Masson,** and myself, have always admitted the possibility that electrolytes possess a certain amount of conduction proper—small indeed, but not so small as to prevent its being evident in certain forms of experiments:

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* MS. letter. † Annales de Chimie, xlii. 257. ‡ Phil. Mag. viii. 465.
§ Bibl. de Genève, xxvi. 134, 144; xxvii. 177. ‖ Comptes Rendus, xxxviii. 897.
¶ Comptes R., xxxvii. 580; or Bibl. de Genève, xxiv. 263; xxv. 180; xxvi. 126.
** Prize Essay, Haarlem Trans., xi. 78.

and beautiful and close as the electrolytic proofs have been carried, they are not by us considered as sufficient to show that the function of conduction proper is altogether absent from electrolytes.

(Some account was then given of the experiments and arguments on both sides; and of the striking electrolytic fact, that if a current of electricity, however small, is sent through a circuit containing a couple of platina plates in dilute sulphuric acid, the plates are found thereby electrically polarized.)

The enquiry as regards electrolytes takes on three forms. They may possess a degree of conduction proper at all times—or they may be absolutely destitute of conduction proper—or they may possess conduction proper up to a certain condition, governed either by requisite intensity for electrolyzation or by other circumstances, but which, when that condition is acquired, changes into electrolytic conduction; and these three forms may be further varied by considerations dependent upon the physical state of the electrolyte, as whether it be solid or liquid, hot or cold, and whether it be pure or contain other substances mingled with it.

From the time when the question was raised by myself, twenty years ago, to the present day, I have found it necessary to suspend my conclusions; for close as the facts have in certain cases been urged by those who believe they have always obtained decomposition results, when an electrolyte has performed the part of a conductor, and freely as I could have admitted the facts and the conclusions if there had been no opposing considerations, still, because there are such considerations, I am obliged to reserve my judgment. In the first place all bodies not electrolytic, even up to gases (Becquerel,) are admitted to possess conduction proper; *a priori*, therefore, we have reason to expect that electrolytes will possess it also. If from amongst different bodies we retain for consideration the class of electrolytes only, then though the amount of electricity of a given intensity which these can transmit electrolytically when they are *fluid*, is often almost infinitely greater than that which they can convey onwards by conduction proper, when they are *solid;* still the conduction in the latter cases is very evident. A piece of perfectly dry solid nitre, and of many other electrolytes, discharges a gold leaf electrometer very freely, and I believe by the power of conduction proper; and that being the case, I do not see that the assumption of the very highest condition of electrolytic conduction when the nitre is rendered fluid is any argument for the absolute disappearance of the conduction proper which belonged to the body in the solid state, though it may override the latter for the time and make it insensible. These considerations are, however, such as arise rather from the absence of the final and strict proof on the opposite side, than from any thing very positive in their own character; but it has occurred to me that the phenomena of static electricity will furnish us with many reasons of a positive nature, in favour of the possession by

liquid electrolytes of the power of conduction proper. Some of these I will endeavour briefly to state, illustrating the subject by a reference to water, which in its pure state has but a low degree of electrolytic conduction.

The ordinary phenomena of static charge and induction are well known. If an excited glass rod or other body be held near a light gilt sphere, suspended from the hand by a metal thread, the inductive action disturbs the disposition of the electricity in the sphere, and the latter is strongly attracted: if in place of the sphere a soap bubble be employed, the same results occur. If a dish filled with pure distilled water be connected with the earth by a piece of moist bibulous paper, and a ball of excited shell-lac be suspended two or three inches above the middle of the water,—and if a plate of dry insulating gutta-percha, about eight inches long and two inches wide, have its end interposed between the water and the shell-lac, it may then be withdrawn and examined, and will be found without charge, even though it may have touched the shell-lac; but if the end once touch the water under the lac (and it may be dipped in,) so as to bring away a film of it, charged with the electricity the water has acquired by the induction, it will be found to possess, as might be expected, a state contrary to that of the inductric shell-lac.

In order to exclude any conducting body but water from what may be considered as a reference experiment, two calico globular bags with close seams were prepared; and being wetted thoroughly with distilled water, were then filled with air by means of a fine blow-pipe point; they were then attached to two suspending bands of gutta-percha, by which they were well insulated, and being about three inches diameter they formed, when placed in contact, a double system six inches in length. A metallic ball, about four inches in diameter, was connected with the electric machine to form an inductric body, an uninsulated brass plate was placed about nine inches off to form an inducteous body; between these the associated water balls could be placed so as to take part in the induction, and when the electric charge was so low that the moist atmosphere caused no transmission of electricity, the balls could be introduced into position and brought away without having received any permanent charge. Under these circumstances if the associated balls were brought into the place of induction, were then separated, withdrawn, and examined, they were found, the one charged positively and the other negatively, by electricity derived from themselves, and without conductive or convective communication with any other substance than their own water.

It is well known indeed that by the use of water we may replace metal in all electro-static arrangements, and so form Leyden jars, condensers, and other induction apparatus, which are perfect in principle though with imperfect action. The principles are the same, whether water or metals be used for conductors, and the

function of conduction is essential to all the results; therefore conduction cannot be denied to the fluid water, which in all such cases is acting as the only conductor. In nature, indeed, the phenomena of induction, rising up to their most intense degree in the thunder-storm, are almost, if not altogether, dependent upon the water which in the earth, or the clouds, or the rain, is then acting by its conducting power; and if this conducting power be of the nature of *conduction proper*, it is probable that that function is as large and as important as any exercise of the electrolytic conduction of water in other natural phenomena.

But it may be said that all these cases, when accompanied by conduction, involve a corresponding and proportionate electrolytic effect, and are therefore cases of electrolytic conduction; and it is the following out of such a thought that makes me think the results prove a conduction proper to exist in the water. For suppose a water bubble to be placed midway between a positive and a negative surface, as in the figure, then the parts at and about p will

become charged positive, and those at and about n negative, solely by the disturbance of the electric force originally in the bubble, *i. e.* without any direct transmission of the electric force from N or P; the parts at e or q will have no electric charge, and from those parts to p and n the charge will rise gradually to a maximum. The electricity which appears at p, n, and elsewhere, will have been *conducted* to these parts from other parts of the bubble; and if the bubble be replaced by two hemispheres of metal, slightly separated at the equatorial parts e q, the electricity (before *conducted* in the continuous bubble,) will then be seen to pass as a bright spark. Now the particles at any part of the water bubble may be considered under two points of view, either as having had a current passed through them, or as having received a charge; in either view the idea of conduction proper supplies sufficient and satisfactory reasons for the results; but the idea of electrolytic conduction seems to me at present beset with difficulties. For consider the particles about the equator e q, they acquire no final charge, and they *have conducted*, as the action of the two half spheres above referred to show; and they are not in a state of mutual tension, as is fully proved by very simple experiments with the half hemispheres. Therefore oxygen must have passed from e towards n, hydrogen from e towards p, *i. e.* towards and to the parts to which the electricity has been conducted, for without such transmission of

the anions and cations there would be no transmission of the electricity, and so no electrolytic conduction. But then the questions arise,—Where do these elements appear? is the water at *n* oxygenated, and that about *p* hydrogenated? and may the elements be at last dispersed into the air at these two points, as in the case of decompositions against air poles? (*Exp. Res.* 455, 461, &c.) In regard to such questions other considerations occur respecting the particles about *p* and *n*, and the condition of charge they have acquired. These have received the electricity which has passed as a current through the equatorial parts, but they have had no current or no proportional current through themselves—the conduction has extended *to* them but not *through* them; no electricity has passed for instance through the particle at *n* or at *p*, yet more electricity has gone by some kind of conduction to them than to any other of the particles in the sphere. It is not consistent with our understanding of electrolytic conduction to suppose that these particles have been charged by such conduction; for in the exercise of that function it is just as essential that the electricity should *leave* the decomposing particle on the one side, as that it should *go to it* on the other: the mere escape of oxygen and hydrogen into the air is not enough to account for the result, for such escape may be freely permitted in the case of electrodes plunged into water; and yet if the electricity cannot pass from the decomposing particles into the electrodes, and so away by the wires, in a condition enabling it to perform its full equivalent of electric work any where else in the circuit, there is no decomposition at the final particles of the electrolyte, nor any electrolytic conduction in its mass. Even in the air cases above referred to there is a complete transmission of the electricity across the extreme particles concerned in the electrolysis.

If the above reasoning involve no error, but be considered sufficient to show that the particles at *p* and *n* are not electrolyzed, then it is also sufficient to prove that none of the particles between *p* and *n* have been electrolyzed; for though one at *e* or *q* may have had a current of electricity passed through it, it could not give up its elements unless the neighbouring particles were prepared to take them in a fully equivalent degree. To stop the electrolysis at *n* and *p*, or at those parts of the surface where the moving electricity stops, is to stop it at all the intervening parts according to our present views of electrolysis, and to stop the electrolysis is to shut out electrolytic conduction; and nothing at present remains but *conduction proper*, to account for the very manifest effects of conduction which occur in the case.

It may be imagined that a certain polarized state of tension occurs in these cases of static induction, which is intermediate between it and electrolytic conduction (*Exp. Res.* 1164); or that a certain preparatory and as it were incomplete condition may be assumed, distinguishing the case of static conduction with globes of water, which I have taken as the ground of consideration from the same

case when presented by globes of metal. Our further and future knowledge may show some such state; but in respect of our present distinctive views of conduction proper and electrolytic conduction, it may be remarked that such discovery is just as likely to coincide with the former as with the latter view, though it most probably would alter and correct both.

Falling back upon the consideration of the particles between *e* and *n*, we find, that whether we consider them as respects the current which has passed through them, or the charge which they have taken, they form a continuous series; the particle at *e* has had most current, that at *n* none, that at *r* a moderate current; and there are particles which must have transmitted every intermediate degree. So with regard to charge; it is highest at *n*, nothing at *e*, and every intermediate degree occurs between the two. Then with respect to these superficial particles, they hold all the charge that exists, and therefore all the electricity which has been conducted is in them; consequently all the electrolytic results must be there; and that would be the case, even though for the shell we were to substitute a sphere of water. For, if those particles which have had more current through them than others be supposed to have more of the electrolytic results about them than the others, then that electricity which is found associated chiefly, if not altogether, with these others, could have reached them only by conduction proper, which for the moment is assumed to be non-existent. So, to favour the electrolytic argument, we will consider the conduction as ending at, and the electrolytic results as summed up in, these superficial particles, passing for the present the former objection that though the electricity has reached, it has not gone through, these particles. Taking, therefore, a particle at *r*, and considering its electrolytic condition as proportionate to the electricity which has arrived at that particle, and given it charge, we may then assume, for we have the power of diminishing the inductive action in any degree, that the electricity, the conduction of which has ceased upon the particle that was there has been just enough to decompose it, and has left what was the under but is now the surface particle, charged. In that case, some other particle, in a higher state of charge, and nearer to *n*, as at *s*, will have had enough electricity conducted towards its place to decompose two particles of water;—but it is manifest that this cannot be the next particle to that at *r*, but that a great number of other particles in intermediate states of charge must exist between *r* and *s*. Now the question is, how can these particles become intermediately charged by virtue of electrolytic conduction only? Electrolytic action is definite, and the very theory of electrolytic conduction assumes that the particles of oxygen and hydrogen as they travel convey not a variable but a perfectly definite amount of power onward in its course, which amount they cannot divide, but must take at once from a like particle, and give at once to another like particle. How

K 2

then can any number of particles, or any action of such particles carry a fraction of the force associated with each particle? It is no doubt true, that if two charged particles can throw their power either on to one, or to three or more other particles, then all the difficulty disappears. Conduction proper can do this: but, as we cannot conceive of a particle half decomposed, so I cannot see how this can be performed by electrolytic conduction, *i.e.* how the particle between *r* and *s* can be excited to the intermediate and indefinite degree, conduction without electrolysis being denied both to it and the particles around it.

If the particles between *e* and *n* be supposed to conduct electrolytically by the current which passes *through* them (dismissing for a time, amongst other serious objections, that already given that the products would not be found at the places to which the electricity has been conveyed) still the present argument would have like force. At *r* enough electricity may have passed through to decompose two particles of water, at *s* only enough to decompose one,—how is a particle between *r* and *s* to change elements with the particles either towards *r* or towards *s*, if electrolytic change only is to be admitted? or how, as before enquired, can two particles throw their power on to, or receive their power from one? Many other considerations spring out of the thought of a water bubble, under static induction; but these just expressed, with those that relate to the *seat* of electrolytic action, whether at the place of current or of charge, create a sum of difficulty fully sufficient, without any others, to make me suspend for the present any conclusions on the matter in question.

The conduction power of water may be considered under another point of view; namely, that which has relation to the absolute charge that can be given to the fluid. A point from the electrical machine can charge neighbouring particles of air, and they issue off in streams. It can do the same to particles of camphine, or oil of turpentine;—it can do the same to the particles of water; and if two fine metallic wires connected with Ruhmkorff's apparatus, be immersed in distilled water, about half an inch apart, the motes usually present will soon show how the water receives charge, and how the charged water passes off in streams, which discharge to each other in the mass. Now such charge is not connected with electrolysis; the condition of electrolyzation is that the electricity pass through the water and do not stop short in it. The mere charge of the water gives us no idea where any constituents set loose by electrolysis can be evolved, and yet conduction is largely concerned in the act of charging. A shower of rain falls across a space in the atmosphere subject to electric action, and each drop becomes charged; spray may be thrown forth from an electrified fountain very highly charged;—conduction has been eminently active in both cases, but I find it very difficult to conceive how that conduction can be electrolytic in its character.

When drops of water, oppositely electrified, are made to approach each other, they act by convection, *i. e.* as carriers of electricity; when they meet they discharge to each other, and the function of conduction is for the time set up. When the water bubble, described p. 5, is taken out of the sphere of induction, the opposite electricities about *p* and *n* neutralize each other, being conducted through the particles of the water. Are we to suppose in these cases that the conduction is electrolytic? if so, where are the constituents separated, and where are they to appear? It must be a strong conviction that would deny conduction proper to electrolytes in these cases; and if not denied here, what reason is there ever to deny it absolutely.

The result of all the thought I can give to the subject is a suspended judgment. I cannot say that I think conduction proper is as yet disproved in electrolytes; and yet I cannot say that I know of any case in which a current, however weak, being passed by platinum electrodes across acidulated water does not bring them into a polarized condition. It may be that when metallic surfaces are present, they complete by their peculiarities the condition necessary to the evolution of elements, which, under the same degree of electrification would not be evolved if the metals were away; and, on the other hand, it also may be that after the metals are polarized, and a consequent state of reactive tension so set up, a degree of conduction proper may occur between them and the electrolyte simultaneously with the electrolytic action. There is now no doubt that as regards electrolysis and its law, all is as if there were but electrolytic conduction; but, as regards static phenomena (which are equally important) and the steps of their passage into dynamic effects, it is probable that conduction proper rules with electrolytes as with other compound bodies: for it is not as yet disproved, is supported by strong presumptive evidence, and may be essential. Yet so distant are the extremes of electric intensity, and so infinitely different in an inverse direction are the quantities that may and do produce the essential phenomena of each kind, that this separation of conductive action may well seem perfect and entire to those whose minds are inclined rather to see conduction proper replaced by electrolytic conduction, than to consider it as reduced, but not destroyed; disappearing, as it were, for electricity of great quantity and small intensity, but still abundantly sufficient for all natural and artificial phenomena, such as those described, where intensity and time both unite in favouring the final results required.

But we must not dogmatise on natural principles, or decide upon their physical nature without proof; and, indeed, the two modes of electric action, the electrolytic and the static, are so different yet each so important, the one doing all by quantity at very low intensity, the other giving many of its chief results by intensity with scarcely any proportionate quantity, that it would be dangerous to deny too hastily the conduction proper to a few cases in static

induction, where water is the conductor, whilst it is known to be essential to the many, only because, when water is the electrolyte employed, electrolytic conduction is essential to every case of electrolytic action.

[M. F.]

WEEKLY EVENING MEETING,

Friday, June 1.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President, in the Chair.

PROFESSOR TYNDALL, F.R.S.

On the Currents of the Leyden Battery.

IN our conceptions and reasonings regarding the forces of nature we perpetually make use of symbols, which, when they possess a high representative value, we dignify with the name of theories. We observe, for example, heat propagating itself through a bar of metal, and help ourselves to a conception of the process by comparing it with water percolating through sand, or travelling by capillary attraction through a lump of sugar. In some such way we arrive at what is called the material theory of heat. The thing seen is thus applied to the interpretation of the thing unseen, and the longing of the human mind to rest upon a satisfactory reason, is in some measure satisfied. So also as regards the subject of the present evening's discourse; we are not content with the mere facts of electricity; we wish to look behind the fact, and prompted by certain analogies we ascribe electrical phenomena to the action of a peculiar fluid. Such conceptions have their advantages and their disadvantages: they afford peaceful lodging to the intellect for a time, but they also circumscribe it; and by and by, when the mind has grown too large for its mansion, it often finds a difficulty in breaking down the walls of what has become its prison instead of its home. Thus, at the present day, the man who would cross the bounds which at present limit our knowledge of electricity and magnetism finds it a work of extreme difficulty to regard facts in their simplicity, or to rid them of those hypothetical adornments with which common consent has long invested them.

But though such is the experience of the earnest student of Natural Philosophy at the present—though he may be compelled to refuse his assent to the prevalent theoretic notions, he may never-

theless advantageously make use of the language of these theories in bringing the facts of a science before a public audience; and in speaking of electricity, the speaker availed himself of the convenient hypothesis of two fluids, without at all professing a belief in their existence. A Leyden jar was charged. The interior of the jar might be figured as covered with a layer of positive electricity, and the exterior by a layer of negative electricity; which two electricities, notwithstanding their mutual attraction, were prevented from rushing together by the glass between them. When the exterior and interior coating are united by a conducting body, the fluids move through the conductor and unite; thus producing what is called an electric current. The mysterious agent which we darkly recognise under this symbol is capable of producing wonderful effects; but one of its most miraculous characteristics is its power of arousing a transitory current in a conductor placed near it. The phenomena of voltaic induction are well known; and it is interesting to inquire whether frictional electricity produces analogous phenomena. This question has been examined by Dr. Henry, and still more recently by that able and experienced electrician M. Riess, of Berlin. The researches of these gentlemen constituted the subject of the evening's discourse.

A wooden cylinder was taken, round which two copper wires, each 75 feet in length, were wound; both wires being placed upon a surface of gutta-percha, and kept perfectly insulated from each other. The ends of one of these wires were connected with a universal discharger, whose knobs were placed within a quarter of an inch of each other; when the current of a Leyden battery was sent through the other wire, a secondary current was aroused in that connected with the discharger, which announced itself by a brilliant spark across the space separating the two knobs.

The wires here used were covered externally with a sheet of gutta-percha; and lest it should be supposed that a portion of the electricity of the battery had sprung from one wire to the other, two flat disks were taken. Each disk contained 75 feet of copper wire, wound in the form of a flat spiral, the successive convolutions of which were about two lines apart. One disk was placed upon the other one, the wire being so coiled that the convolutions of each disk constituted, so to say, the impress of those of the other, and the coils were separated from each other by a plate of varnished glass. The ends of one spiral were connected with the universal discharger, between whose knobs a thin platinum wire, ten inches long, was stretched. When the current of the Leyden battery was sent through the other spiral, the secondary current, evoked in the former, passed through the thin wire, and burnt it up with brilliant deflagration. A pair of spirals were next placed six inches apart, and a battery was discharged through one of them; the current aroused in the other was sufficient to deflagrate a thin platinum wire four inches in length.

We have every reason to suppose that the secondary current thus developed is of the same nature as the primary which produced it; and hence we may infer, that if we conduct the secondary away and carry it through a second spiral, it, in its turn, will act the part of a primary, and evoke a *tertiary* current in a spiral brought near it. This was illustrated by experiment. First, two spirals were placed opposite to each other, through one of which the current of the battery was to be sent; the other was that in which the secondary current was to be aroused. The ends of the latter were connected by wires with a third spiral placed at a distance, so that when the secondary current was excited it passes through the third spiral. Underneath the latter, and separated from it by a sheet of varnished glass, was a fourth spiral, whose two ends were connected with the universal discharger, between the knobs of which a quantity of gun-cotton was placed. When the battery was discharged through the first spiral, a secondary current was aroused in the second spiral, which completed its circuit by passing through the third spiral: here the secondary acted upon the spiral underneath, developed a tertiary current which was sufficiently strong to pass between the knobs, and to ignite the gun-cotton in its passage. It was shown that we might proceed in this way and cause the tertiary to excite a current of the fourth order, the latter a current of the fifth order, and so on; these children, grandchildren, and great grandchildren of the primary being capable of producing all the effects of their wonderful progenitor.

The phenomena of the *extra current*, which exists for an instant contemporaneously with the ordinary current in a common voltaic spiral, were next exhibited; and the question whether a spiral through which a Leyden battery was discharged exhibited any similar phenomena was submitted to examination. It was proved, that the electric discharge depended upon the *shape* of the circuit through which it passed: when two portions of such a circuit are brought near each other, so that the positive electricity passes in the same direction though both of them, the effect is that the discharge is *weaker* than if sent through a straight wire: if, on the contrary, the current flow through both portions in opposite directions the discharge is *stronger* than if it had passed through a straight wire. A flat spiral was taken, containing 75 feet of copper wire; one end of the spiral was connected with a knob of the universal discharger, and the other knob was connected with the earth: between the knobs of the discharger about four inches of platinum wire were stretched; on connecting the other end of the spiral with the battery a discharge passed through it of such a strength that it was quite unable to raise the platinum wire to the faintest glow. The same length of copper wire was then bent to and fro in a zigzag manner, so that on every two adjacent legs of the zigzag the current from the battery flowed in opposite directions. When these 75 feet of wire were interposed between the battery and the platinum wire, a discharge

precisely equal to that used in the former instance, raised the platinum wire to a high state of incandescence, and indeed could be made to destroy it altogether.

When a primary and a secondary spiral are placed opposite to each other, a peculiar reaction of the secondary upon the primary is observed. If the ends of a secondary (50 feet long) be connected by a thick wire, the effect upon the primary current is the same as when the ends of the secondary remain wholly unconnected. If the ends of the secondary be joined by a long thin platinum wire, the reaction of the secondary is such as to enfeeble the primary. This enfeeblement increases up to a certain limit as the resistance is increased, from which forwards it diminishes until it becomes insensible. This would appear to prove that to react upon the primary the secondary requires to be retarded; and that the greater the amount of the retardation, up to a certain limit, the greater is the enfeeblement. But by increasing the resistance we diminish the strength of the secondary, and when a certain limit is attained, this diminution is first compensated for by the influence of retardation, from which point forwards with every increase of the resistance, the enfeeblement of the primary is diminished. A primary current which fuses a certain length of platinum wire where the ends of the secondary are disunited, or where they are united by a thick wire, fails to do so when they are united with a thin wire. But if, instead of a thin wire, a body of much greater resistance, a column of water for example, be introduced, the platinum wire is fused as before.

[J. T.]

GENERAL MONTHLY MEETING,

Monday, June 4.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President, in the Chair.

J. S. Coleman, Esq.
Wm. De Lannoy, Esq.
George H. Ingall, Esq.
Col. William Kirkman Loyd.
R. Bentley Todd, M.D. F.R.S.

were duly *elected* Members of the Royal Institution.

George Ade, Esq.

was *admitted* a Member of the Royal Institution.

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM

Administration of the Mines of Russia—Compte Rendu Annuel, 1853. Par A. T. Kupffer. 4to. St. Petersburg. 1854.

Asiatic Society of Bengal—Journal, No. 246. 8vo. 1855.

Astronomical Society, Royal—Monthly Notices. Vol. XV. No. 6. 8vo. 1855.

Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for May, 1855. 8vo.

Blashfield, J. M. Esq. M.R.I. (the Author)—History and Manufacture of Terra-Cotta, Ancient and Modern. 8vo. 1855.

Boosey, Messrs. (the Publishers)—The Musical World for May, 1855. 4to.

British Architects, Royal Institute of—Proceedings in May, 1855. 4to.

Civil Engineers, Institution of—Proceedings in May, 1855. 8vo.

Dilke, C. Wentworth, Esq. (the Author)—Catalogue of a Collection of Works on, or having reference to, the Exhibition of 1851, in the possession of C. W. Dilke. 8vo. 1855.

Editors—The Medical Circular for May, 1855. 8vo.
The Practical Mechanic's Journal for May, 1855. 4to.
The Journal of Gas-Lighting for May, 1855. 4to.
The Mechanics' Magazine for May, 1855. 8vo.
Deutsches Athenäum for May, 1855. 4to.
The Athenæum for May, 1855. 4to.

Faraday, Professor, D.C.L. F.R.S.—Monatsbericht der Königl. Preuss. Akademie, für April, 1855. 8vo. Berlin.
Whitelocke, B.—Journal of the Swedish Ambassy in 1653-4, from the Commonwealth of England, Scotland, and Ireland, written by the Ambassador, the Lord Commissioner Whitelocke. 2 vols. 4to. 1762.
Shermann, A. J.—Historia Collegii Jesu Cantabrigiensis, edidit et notis instruxit J. O. Halliwell. 8vo. 1840.

Faraday, Professor, D.C.L. F.R.S—Sedgwick, A.—Discourse on the Studies of the University. 8vo. 1834.
Jardine, G.—Outlines of Philosophical Education. 8vo. Glasgow, 1818.
Deleuze, J. P. F.—Eudoxe : Entretiens sur l'Etude des Sciences, des Lettres et de la Philosophie. 2 vols. 8vo. Paris, 1810.
Memorie della Reale Accademia della Scienze di Torino. Serie Seconda. Tomo XIV. 4to. Torino, 1854.
Macvicar, J. G.—Elements of the Economy of Nature, or the Principles of Physics, Chemistry, and Physiology. 8vo. 1830.
Tryon, T.—Letters on Subjects Philosophical, Theological, and Moral. 12mo. 1700.
Howell, J. W.—The Unity of Nature. 8vo. 1849.
Leeuwenhoek, A.—Arcana Naturæ ope et beneficio Microscopiorum detecta. 4to. Lug. Bat. 1696.
Addison, W.—The actual Process of Nutrition in the Living Structure, demonstrated by the Microscope, &c. 8vo. 1843.
Hales, Stephen.—Statical Essays : Vegetable Statics and Hæmastatics. 3rd Ed. 2 vols. 8vo. 1738-1740.
Varley, Cornelius.—A Treatise on Optical Drawing Instruments. 8vo. 1845.
Vallée, L. L.—Théorie de l'Œil, Première Partie. 8vo. 1844–6.
Reynaud, A. A. L.—Elémens d'Algèbre. 3e Ed. 2 vols. 8vo. Paris, 1810.
Elémens des Sections Coniques. Par M. M**. 8vo. Paris, 1757.
Institutions de Physique. 8vo. 1741.
Nollet, J. A.—Leçons de Physique Expérimentale. 4e Ed. 5 vols. 12mo. Paris, 1753-5.
Gay-Lussac, J. L.—Cours de Physique. 8vo. Paris, 1827.
Baily, F.—Astronomical Tables and Formulæ. 8vo. 1827.
Boyle, R.—New Experiments touching the Spring of the Air and its effects (made for the most part in a new Pneumatical Engine). 4to. 1662.
Berzelius, J. J.—Nouveau Système de Minéralogie. 8vo. Paris, 1819.
La Platine, l'or blanc ou le Huitième Métal. 12mo. Paris, 1758.
De Born, Ignace.—Méthode d'extraire les Métaux parfaits des Minérais par le Mercure. 4to. Vienne, 1788.
Bouillon-Lagrange, E. J. B.—Essai sur les Eaux Minérales Naturelles et Artificielles. 8vo. Paris, 1811.
Wardrop, J.—On Diseases of the Heart. 8vo. 1851.
Royle, J. F.—On the Antiquity of Hindoo Medicine. 8vo. 1837.
Berthier, M. P.—Mémoires et Notices Chimiques, Minéralogiques, &c. 8vo. Paris, 1833.
Le Bègue de Presle—Le Conservateur de la Santé. 12mo. Paris, 1763.
Berzelius, J. J.—Essai sur la Théorie des Proportions Chimiques. 8vo. Paris, 1819.
Chevreul, E.—Considérations générales sur l'Analyse Organique. 8vo. Paris, 1824.
Elémens de Chimie Théorique et Pratique, pour servir aux cours public de l'Académie de Dijon. 3 vols. 12mo. 1777-8.
Collectanea Chymica—A collection of several Treatises in Chymistry concerning the Liquor Alkahest, the Mercury of the Philosophers, &c. 8vo. 1684.
Gay-Lussac, J. L.—Cours de Chimie. 2 vols. 8vo. Paris, 1828.
De Nuisement, Le Sieur—Traittez du Vray Sel, secret des Philosophes et de l'Esprit Général du Monde. 12mo. Paris, 1621.
Laugier, M.—Chimie Générale. 3 vols. 8vo. 1828.
Low, D.—Inquiry into the Nature of the Simple Bodies of Chemistry. 2nd Ed. 8vo. 1848.
Louyet, P.—Cours Elémentaire de Chimie Générale. 3 vols. 8vo. Bruxelles, 1841-4.
Macquer, M.—Dictionnaire de Chymie ; avec une Supplément, par M. H. Struve. 5 vols. 8vo. Neuchatel, 1789.

Faraday, Professor, D.C.L. F.R.S.—Pictet, M. A.—Essais sur le Feu. 8vo. Genève, 1790.

Pött, J. H.—Dissertations Chymiques, recueillies et traduites, par J. F. Demachy. 4 vols. 12mo. Paris, 1759.

Recueil de Mémoires et d'Observations sur la Formation et sur la Fabrication du sal pêtre. Par les Commissaires nommés par l'Académie pour le jugement du prix du sal pêtre. 8vo. Paris, 1776.

Ségur, Octave.—Lettres Elémentaires sur la Chimie. 2 vols. 18mo. 1803.

Stahl, G. E.—Traité des Sels. 18mo. Paris, 1783.

Otto Tachenius.—Hippocrates Chymicus. Translated by J. W. 4to. 1677.

Brück, R.—Electricité ou Magnétisme du Globe Terrestre. 8vo. Bruxelles, 1851.

Bostock, J.—An Account of the History and Present State of Galvanism. 8vo. 1818.

Carpue, J. C.—An Introduction to Electricity and Galvanism. 8vo. 1803.

Nollet, J. A.—Lettres sur l'Electricité. Nouvelle Ed. 2 vols. 12mo. Paris, 1764.

Guglielmini, D.—Della Natura de Fiumi. 4to. Bologna, 1697.

Smyth, Capt W. H.—Nautical Observations on the Port and Maritime Vicinity of Cardiff, and on the Bute Docks. 8vo. 1840.

Theophrastus.—History of Stones, by J. Hill: with two Letters. 8vo. 1746.

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Burnet, T.—The Theory of the Earth. 3rd Ed. fol. 1697.

Smith, J. Pye—On the Relation between the Holy Scriptures and some Parts of Geological Science. 8vo. 1839.

Schlagintweit, H. und A.—Untersuchungen über die Physikalische Geographie der Alpen. 4to. 1850.

Knight, W.—Facts and Observations towards forming a New Theory of the Earth. 8vo. 1818.

Eaton, A.—Geological Text-book (for America.) 8vo. Albany, U.S. 1830.

Ainsworth, W.—Account of the Caves of Ballybunian in the county of Kerry. 8vo. Dublin, 1834.

Exposition des Produits de l' Industrie Française en 1839—Rapport du Jury central. 3 vols. 8vo. Paris, 1839.

Treatise on Calico Printing. 12mo. 1793.

Black, W.—A Practical Treatise on Brewing. 8vo. 1835.

Moseley, B.—A Treatise on Sugar. 8vo. 1799.

Curr, J.—Railway Locomotion and Steam Navigation: their Principles and Practice. 8vo. 1847.

Hall, Mr.—The principal Roots of the Latin Language. 8vo. 1825.

Roberts, T.—An English and Welsh Vocabulary. 12mo. 1827.

Forde, W.—The True Spirit of Milton's Versification developed. 8vo. 1831.

Schweigger, J. S. C.—Einleitung in die Mythologie. 8vo. Halle, 1836.

Fellows, Sir Charles, V.P.R.I. (*the Author*)—Coins of Ancient Lycia before the reign of Alexander: with an Essay on the relative dates of the Lycian Monuments in the British Museum. 8vo. 1855.

Franklin Institute of Pennsylvania—Journal, Vol. XXIX. Nos. 4, 5. 8vo. 1855.

Geological Society—Quarterly Journal, No. 42. 8vo. 1855.

Graham, George, Esq. Registrar-General—Reports of the Registrar-General for May, 1855. 8vo.

Hamilton, W. J. Esq. Pres. Geol. Soc. (*the Author*)—Address at the Anniversary Meeting of the Geological Society, Feb. 16, 1855. 8vo. 1855.

Hopkins, Thomas M. Esq. (*the Author*)—On the Atmospheric Changes which produce Rain and Wind. 2nd Ed. 8vo. 1854.

Horticultural Society of London—Journal, Vol. IX. Part 4. 8vo. 1855.

Ingall, G. H. Esq.—The History of Britain. By John Milton. 4to. 1670.

Jennings, Richard, Esq. M.A. M.R.I. (the Author)—Natural Elements of Political Economy. 16mo. 1855.

London, Library Committee of the Corporation—Catalogue of London Traders' Tokens—Beaufoy Cabinet. By J. H. Burn. 2nd Ed. 8vo. 1855.

Manning, Frederick, Esq. M.R.I.—The Life of Thomas Ken, Bishop of Bath and Wells. By a Layman. 2nd Ed. 2 vols. 8vo. 1854.

Approach to the Holy Altar. By Bishop Ken. 16mo. 1854

Exposition of the Apostles' Creed. Bp. Bishop Ken. 16mo. 1854.

Novello, Mr. (the Publisher)—The Musical Times for May, 1855. 4to.

Phillipps, Sir Thomas, Bart. F.R.S. F.S.A. M.R.I. (the Author)—Index Nominum in Libris Dictis Cole's Escheats. 16mo. 1852.

Photographic Society—Journal, No. 30. 8vo. 1855.

Radcliffe Trustees, Oxford—Astronomical and Meteorological Observations made at the Radcliffe Observatory, in 1853. 8vo. 1855.

Royal Society.—Proceedings, Vol. VII. No. 12. 8vo. 1855.

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Berichte, Phil.-Hist. Classe. 1854. Heft 2–6; 1855. Heft 1, 2. 8vo.

Gedächtnissrede auf Friedrich August, König von Sachsen. Von E. von Wietersheim. 8vo. 1854.

Scharf, George, Esq. jun. F.S.A. (the Author)—Notes upon the Sculptures of a Temple discovered at Bath, in 1790. 4to. 1855.

Society of Arts—Journal for May, 1855. 8vo.

Statistical Society—Journal, Vol. XVIII. Part 2. 8vo. 1855.

Wrey, J. W. Esq. M.A. M.R.I.—Explanations: a Sequel to "Vestiges of the Natural History of Creation." By the Author of that work. 12mo. 1846.

WEEKLY EVENING MEETING,

Friday, June 8.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On Ruhmkorff's Induction Apparatus.

This apparatus is known to consist of a soft iron core, intended to act magnetically, around which there is a coil of coarse copper wire, to be connected at pleasure with few or many cells of a voltaic battery; and external to this is a second coil of much thinner copper wire, having great length, in which the peculiar currents of the apparatus are to be produced. The coils of the wires are insulated from one another by a very careful mode of preparation. The inner, called the primary coil, is supplied with an automatic contrivance, so that when the battery is connected with it, the continuity is broken, to be renewed again an instant after; and thus a

series of short intermitting currents, rapidly recurring, pass through it instead of one constant current. The outer coil, usually called the secondary, has its terminations apart; and these can be connected either metallically by a wire, or arranged with any interval or apparatus placed between them, in which effects of the induced current are to be shown, and by which its characters are to be examined.

When the secondary circuit is metallically complete, each brief current in the primary wire causes, according to well-known principles (*Exp. Res.* 10, &c.), two successive currents in opposite directions in the secondary wire; and if a galvanometer be included in the secondary circuit, it is seen that the communication of the primary wire with the battery is followed by a deflection of the needle in one direction, which then gradually swings to and fro, accompanied by curious spasmodic motions (which are understood upon a moment's inspection), until it comes to zero: if the primary current be then stopped, the galvanometer needle is deflected in the other direction, and after a few oscillations subsides quietly to zero again. The sum of the alternate induction currents having been thus shown to be equal in effect to zero, it was then explained how, if the secondary currents be interrupted in the smallest degree, even by the intervention of a hair or a piece of paper, all the currents of one kind, due to the beginnings of the short inducing currents in the primary wire, were stopped off from the secondary wire (being expended in the primary wire itself), and only those due to the cessations of the primary currents left to show their power there; so that the secondary wire could then give a continuous series of intermitting (but not alternating) currents, all of which, therefore, had a common direction.

The remarkable character of the electricity of these currents was then shown and explained. Its intensity is such that it can strike across $\frac{1}{3}$ or $\frac{1}{2}$ an inch of air, whilst the intensity of the inducing current is so feeble that it cannot traverse any sensible striking distance: but it was also shown, that the more intense the electricity the less the sum of force transmitted in a given time by the action of the same battery and apparatus; and that when the interruption of the secondary circuit was the smallest possible, as by a hair's-breadth, the largest amount of electricity passed through the galvanometer connected with it. The power of the induced current to pass through six inches or more of rarified air, was shown in the form of Gassiot's cascade:* and the conversion of the dynamic force of the primary and inducing current into the static force of the induced electricity, was illustrated by the charging of electrometers and Leyden jars.

When the secondary current is interrupted, as just described, the inducing power of the primary current acts in its own wire to pro-

* Phil. Mag. 1854, vii. p. 99.

duce certain hurtful or wasteful results. Fizeau, by applying a Leyden jar (or its equivalent,) to the parts of the primary circuit near the contact breaker, took up this extra power at the moment of time, and converted it to a useful final purpose, upon principles belonging to static induction, the effects of which were briefly explained. Masson,* Grove,† and Sinsteden have made a like application to the terminals of the secondary wire; and Grove has pointed out striking changes in the character of the currents in it thus produced, and useful applications of the results. For instance, the spark in air between the ends of two platinum wires connected with the secondary terminals is flame-like, soft, and comparatively quiet compared with that which is produced when the terminals are respectively connected with the inner and outer coatings of a Leyden jar; for then it becomes very bright, sonorous, and apparently large, so that two sparks can hardly differ more than the same spark under these circumstances. The differences are even greater than the appearances show; for whilst the powerful rattling spark cannot fire wood, or paper, or even gunpowder, except by the use of expedients, the soft quiet spark at once inflames any of them. The effect of the static induction thus introduced is not so much to vary the *quantity* of electricity which passes, as the *time* of the passage. That electricity which, moving with comparative slowness through the great length of the secondary coil, produces a spark having sensible duration (and therefore in character like that of a Leyden jar passed through a wet thread,) is, when the jar is used, first employed in raising up a static induction charge, which when discharged produces a concentrated spark of no sensible duration, and therefore much more luminous and audible than the former. Fixing a piece of platinum wire horizontally across the ball of a Leyden jar, and then bringing the platinum wire secondary terminals respectively near its ends, two interruptions are produced in the secondary circuit, the sparks at which are like each other and equal in quantity of electricity, for the jar as yet forms only an insulating support. But if, in addition, either secondary terminal be connected by a wire with the outside of the jar, the spark on that side assumes the bright loud character before described, but ceases to fire gunpowder or wood; and no one would at first suppose, what is the truth, that there is the same electricity passing in one as in the other.

Another interesting effect of the static induction is the double spark. If one of the secondary terminals be connected with the outside of a Leyden jar, and the other be continued until near the knob or a wire connected with it, a soft spark appears at that interval for every successive current in the primary circuit. This spark, however, is double; for the electricity thrown into the jar at the

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* Prize Essay, Haarlem Trans. 1854, pp. 46, 47.
† Phil. Mag. Jan. 1855, ix. p. 1.

moment of induction, is discharged back again at the same place the instant the induction is over; the first discharge heats and prepares the air there for the second discharge, and the two are so nearly simultaneous as to produce the appearance of a single spark to the unaided eye.

Reference was then made to the hopes raised by this instrument, of advance in the investigation of the magneto-electric power, by means of the great aid which it seems competent to supply. The results obtained by Grove* apparently referable to polarization were adverted to; as also the remarkable transverse bands presented in the recurring discharge across very rarified air†; and, founded as the instrument is by its core and its wires upon the joint effects of electro-dynamic and magneto-electric induction, it was observed that it gave great promise of aid in the investigation of that condition of either the space or the ether which is about magnets, and around every discharge of electricity, whether in good or bad conductors, and which is expressed by the terms (themselves synonymous) of the magnetic or the electrotonic state.

[M. F.]

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* Phil. Trans. 1852, p. 93, &c.

† Phil. Mag. 1852, iv, p. 514.

Royal Institution of Great Britain.

1855.

WEEKLY EVENING MEETING,

Friday, June 15.

H.R.H. The Prince Albert, K.G. F.R.S. Vice-Patron R.I., in the Chair.

Colonel H. C. Rawlinson,

On the Results of the Excavations in Assyria and Babylonia.

These excavations, independently of the treasures of art disclosed by them, have opened up to us a period of about 2000 years in the world's history, which, as far as the East is concerned, was before almost entirely unknown. The cuneiform inscriptions of Babylonia and Assyria furnish a series of historical documents from the 22nd century B.C. to the age of Antiochus the Great. The speaker divided these documents into three distinct periods of history, the Chaldæan, the Assyrian, and the Babylonian, and he then proceeded briefly to describe each period in succession. During the Chaldæan period the seat of empire was to the south, towards the confluence of the Tigris and Euphrates, and the sites of the ancient capitals were marked by the ruins of Mugheir, of Warka, of Senkereh, and of Niffer. At Mughier, called in the inscriptions *Hur*, and representing the biblical *Ur* of the Chaldees, inscriptions have been found of a king, "*Kudur*, the conqueror of Syria," who was probably the Chedorlaomer of the Bible. At any rate, a king named *Ismi-Dagan*, who lived some generations later, is proved, by a series of chronological dates found in the Assyrian tablets, to belong to the 19th century B.C., so that the era of the earlier king agrees pretty well with the ordinary computation of the age of Abraham. The names of about twenty-five kings have been recovered of the ancient period, and there are good grounds for believing that the Assyrians did not succeed in establishing an independent empire at Nineveh till the early part of the fifteenth century B.C.

From B.C. 1273 to 625, the Assyrians seem to have been the lords paramount of Western Asia, and their history is preserved

in an almost continuous series of documents, from the institution of the empire to the taking of Nineveh by the Medes and Babylonians. During the later part of this period, or from about 800 B.C., Jewish history runs in a parallel line with that of Assyria; and wherever a comparison can be instituted between the sacred records and the contemporary annals of Nineveh, the most complete agreement is discovered between them; and that not only in regard to the names of the kings, but also in respect to their order of succession, their relationship to each other, the wars in which they were engaged, and even the leading features of those wars. Col. Rawlinson noticed many such examples of coincidence, and drew attention to the great value of the verification which was thus obtained of Scripture history.

The third, or Babylonian period, was then shortly discussed; the reigns of Nebuchadnezzar and Nabonidus being especially selected for illustration. A description was given of the excavation of the great ruin near Babylon called Birs Nimrud, and a translation was read of the edict of Nebuchadnezzar inscribed upon the clay cylinders, which were found imbedded in the walls of the temple. A number of original relics, discovered among the ruins of Chaldæa, Assyria, and Babylonia, and illustrative of these three periods of history, were also exhibited to the meeting, previously to their being deposited in the British Museum.

LIST OF KINGS.

I. Chaldæan Period.

Name of King.	*Approximate Date.*
Urukh	B.C. 2234
Ilgi	
. . . . .	.
Sinti-Shil-Khak	
Kudur-Mapula	1950
. . . . .	.
Ismi-Dagan	1860
Ibil-Anu-Duma	
Gurguna	
. . . . .	.
Naram-Sin	
Durri-Galazu	1700
Purna-Puriyas	
. . . . .	.
Khammurabi	
Samshu-Iluna	1600
. . . . .	.
Sin-Shada	
Rim-Sin	B.C. 1500
Zur-Sin	
. . . . .	.
Merodach-Gina	
. . . . .	.
. . . . .	1400
. . . . .	.
. . . . .	1300
. . . . .	.

II.—Assyrian Period.

Belukh	1273
Pudil	1255
Phulukh I.	1240
Shalama-Bar I.	1220
Sanda-Pal-Imat	1200
Asshur-Dapal-Il	1185
Mutaggil-Nebo	1165
Asshur-Rish-Ipan	1140

Name of King.	*Approximate Date.*
Tiglath-Pileser I.	B.C. 1120
Asshur-Bani-Pal I.	1100
. . . .	.
Asshur-Adan-Akhi	950
Asshur-Danin-Il	925
Phulukh II.	900
Tigulti-Sanda	880
Sardanapalus	850
Shalama-Bar II. (Asshur-Danin-Pal)	815
Shamas-Phul	780
Phulukh III. Samuramit (or Pul and Semiramis)	760
Tiglath-Pileser II.	747
Shalmaneser (?)	730

Name of King.	*Approximate Date.*
Sargon	B.C. 721
Sennacherib	702
Esar-haddon	680
Asshur-Bani-Pal II.	660
Asshur-Emit-Ilut	640 to 625

III.—Babylonian Period.

Nabopolassar	625
Nabokodrossor (or Nebuchadnezzar)	605
Evil-Merodach	562
Nergal-Shar-Ezer	560
Nabonidus, and Bel-Shar-Ezer (Belshazzar)	554 to 538
Taking of Babylon, by Cyrus.	

N.B.—It must be understood that the reading of many of these names is still far from certain.

[H. R.]

GENERAL MONTHLY MEETING,

Monday, July 2.

Sir Charles Fellows, Vice-President, in the Chair.

Thomas Pargiter Dickenson, Esq.
Thomas Dunn, Esq.
John MacLennan, M.D. and
Captain Raymond White,

were duly *elected* Members of the Royal Institution.

Eustace Anderson, Esq.
J. Richard Andrews, Esq.
John Sherard Coleman, Esq.
William Delannoy, Esq. and
Col. William Loyd,

were *admitted* Members of the Royal Institution.

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM

Airy, G. B. Esq. F.R.S. Astronomer-Royal.—Report on Greenwich Observatory. June 2, 1855. 4to.

Astronomical Society, Royal—Monthly Notices. Vol. XV. No. 7. 8vo. 1855.

Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for June, 1855. 8vo.

Bertie, Hon. and Rev. Frederick.—Five Generations of a Loyal House—Part I.—Lives of Richard Bertie, and his son Peregrine Lord Willoughby. By Lady Georgina Bertie. 4to. 1845.

Boosey, Messrs. (the Publishers)—The Musical World for June, 1855. 4to.

Bradbury, Henry, Esq. M.R.I.—The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Part 3. fol. 1855.

British Architects, Royal Institute of—Proceedings in June, 1855. 4to.

Coleman, J. Sherard, Esq. M.R.I.—Journal of the Ethnological Society of London. Vols. I–III. 8vo. 1848–54.

Commissioners in Lunacy.—Ninth Report. 8vo. 1855.

Diamond, Hugh W. M.D. M.R.I.—Fac-simile of a Letter from Sir H. Davy to Sir F. Baring, dated October 3, 1805 (relating to the London Institution Museum).

East India Company, Hon.—Fibrous Plants of India, fitted for Cordage and Paper. By J. Forbes Royle, M.D. F.R.S. 8vo. 1855.

Editors—The Medical Circular for June, 1855. 8vo.

The Practical Mechanic's Journal for June, 1855. 4to.

The Journal of Gas-Lighting for June, 1855. 4to.

The Mechanics' Magazine for June, 1855. 8vo.

Deutsches Athenäum for June, 1855. 4to.

The Athenæum for June, 1855. 4to.

Faraday, Professor, D.C.L. F.R.S.—Kaiserliche Akademie der Wissenschaften, Wien:—

Philosophisch-Historische Classe — Sitzungsberichte. Band XIII. Heft 3; Band XIV. und Band XV., Heft 1. 8vo. 1855.

Archiv fur Kunde Œsterreichischer Geschichtsquellen. Band XIV. Heft 1. 8vo. 1855.

Notizenblatt, 1855. Nos. 1–12. 8vo.

Mathematisch-Naturwissenschaftliche Classe:—

Denkschriften. Band VIII. 8vo. 1854.

Sitzungsberichte. Band XIV.; und Band XV. Heft 1, 2. 8vo. 1854–5.

Almanach für 1855. 16mo.

Geographical Society, Royal.—Journal, Vol. XXIV. 8vo. 1854.

Graham, George, Esq. Registrar-General—Report of the Registrar-General for June, 1855. 8vo.

James, Lieut.-Col. H. R.E.—Abstracts of the Meteorological Observations at the Stations of the Royal Engineers, in 1853–4. 4to. 1855.

Novello, Mr. (the Publisher)—The Musical Times for June, 1855. 4to.

Petermann, A. Esq. (the Author)—Karte vom Sud-Westl. Russland. 1855.

Mittheilungen auf dem Gesammtgebiete der Geographie. Heft 1, 2, 3, 4. 4to. Gotha, 1855.

Photographic Society—Journal, No. 31. 8vo. 1855.

Royal Society.—Proceedings, Vol. VIII. No. 13. 8vo. 1855.

Society of Arts—Journal for June, 1855. 8vo.

Society for improving the Condition of the Insane—Rules and List of Members, and Prize Essay on the Changes in the Management of the Insane; By D. H. Tuke, M.D. 8vo. 1854.

Taylor, Rev. W.—Magazine for the Blind. July, 1855.

GENERAL MONTHLY MEETING,

Monday, November 5.

Sir GEORGE POLLOCK, G.C.B., Vice-President, in the Chair.

Delamore Jubilee Bailey, Esq.
Archibald Campbell, Esq.
John Evelyn Denison, Esq. M.P.
R. Dick, Esq.
Thomas Farquhar Hill, Esq. and
Jonathan Rigg, Esq.

were duly *elected* Members of the Royal Institution.

William Chapman, Esq.
Thomas Dunn, Esq. and
John MacLennan, M.D.

were *admitted* Members of the Royal Institution.

The Managers reported, That on July 2nd, they appointed THOMAS HENRY HUXLEY, Esq. F.R.S. to fill the vacant office of Fullerian Professor of Physiology.

The Secretary having reported that the three specimens of Nature-Printing accompanying the abstract of Mr. H. BRADBURY's Discourse on May 11 last, were presented by that gentleman, the special thanks of the Members were returned to him for his valuable donation.

The special thanks of the Members were also returned to DECIMUS BURTON, Esq. and R. HUTTON, Esq., executors of the late GEORGE GREENOUGH, Esq., for their present of a copy of Mr. GREENOUGH's "Physical and Geological Map of India," in six sheets; and to WILLIAM NEWTON, Esq. for his present of a copy of his Map and Memoir of "London in the Olden Time."

The following PRESENTS were announced, and the thanks of the Members returned for the same :—

FROM

Accademia de, Nuovi Lincei, Roma—Atti, Anno I. Tomo I.; Anno IV.; Anno V.; Sessioni 1-6. 4to. 1851-3.

Actuaries, Institute of—The Assurance Magazine, Nos. 20, 21. 8vo. 1855.

Agricultural Society of England, Royal—Journal, Vol. XVI. Part 1. 8vo. 1855.
American Academy of Arts and Sciences—Proceedings, Vol. III. Nos. 14–23. 8vo. 1854–55.
American Philosophical Society—Proceedings, No. 51. 8vo. 1855.
Amsterdam, Koninklijke Akademie van Wetenschappen — Verhandelingen. Deel II. 4to. 1855.
Verslagen en Mededeelungen, Deel II. Stuk 3. Deel III. 8vo. 1854-5.
Catalogus der Boekerij. Aflevering I. 8vo. 1855.
Antiquaries, Society of—Proceedings. No. 41, 42. 8vo. 1854–5.
Archæologia, Vol. XXXVI. Part 1. 4to. 1855.
Arnold, Thomas J. Esq., Life Sub. R.I. (the Author)—Reynard the Fox, after the German version of Goethe. With illustrations by J. Wolf. 8vo. 1855.
Arnott, Neil, M.D. F.R.S. (the Author)—On the Smokeless Fireplace, Chimney-valves, and other means of obtaining Healthful Warmth and Ventilation. 8vo. 1855.
Asiatic Society of Bengal—Journal, No. 247-249. 8vo. 1855.
Asiatic Society, Royal—Journal, Vol. XV. Part 2. 8vo. 1854.
Vestiges of Assyria, 3 Maps, by F. Jones.
Astronomical Society, Royal—Monthly Notices, 1855, No. 8. 8vo.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal, August to November, 1855. 8vo.
Bombay Medical Board—Deaths in Bombay in 1853. 4to.
Boosey, Messrs. (the Publishers)—The Musical World, July to Oct., 1855. 4to.
Boston Society of Natural History—Proceedings, Vol. IV., Nos. 25, 26.; Vol. V. Nos. 1-11. 8vo. 1854-5.
Bradbury, Henry, Esq. M.R.I.—The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Parts 4–7. fol. 1855.
British Architects, Royal Institute of—Proceedings in July, 1855. 4to.
Papers read in Session 1854–5. 4to.
British Association—Report of the Twenty-fourth Meeting held at Liverpool in 1854. 8vo. 1855.
Burton, D. & R. Hutton, Esqs. (Executors of the late G. B. Greenough, Esq.) Physical and Geological Map of India. By G. B. Greenough, Esq. F.R.S.
Chemical Society—Quarterly Journal, No. 30. 8vo. 1855.
De la Rive, Prof. A. (the Author)—On the Cause of the Aurora Borealis. 8vo. 1855.
Editors—The Medical Circular for July to October, 1855. 8vo.
The Athenæum for July to October, 1855. 4to.
The Practical Mechanic's Journal for July to October, 1855. 4to.
The Journal of Gas-Lighting for July to October, 1855. 4to.
The Mechanics' Magazine for July to October, 1855. 8vo.
Newton's London Journal, for July to October, 1855.
Ethnological Society of London—Address by John Conolly, M.D. followed by a Sketch of the recent Progress of Ethnology, by R. Cull. 8vo. 1855.
Regulations. 8vo. 1855.
The Ethnological Exhibitions of London. By J. Conolly, M.D. 8vo. 1855.
Probable Origin of the American Indians, by J. Kennedy, Esq. 8vo. 1854.
Faraday, Professor, D.C.L. F.R.S.—Monatsbericht der Königl. Preuss. Akademie, Mai zu August, 1855. 8vo. Berlin.
Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin. 1854. 4to. 1855.
Kaiserliche Akademie der Wissenschaften, Wien:—
Philosophisch-Historische Classe:—Sitzungsberichte, Band XV. Hefte 2, 3; Band XVI. Heft 1. 8vo. 1855.
Denkschriften. Band VI. 4to. 1855.
Mathematisch-Naturwissenschaftliche Classe:—Denkschriften. Band IX. 4to. 1855.

Faraday, Professor, D.C.L. F.R.S.—Sitzungsberichte. Band XV. Heft 3.; Band XVI. Heft 1. 8vo. 1855.

Observations de Phénomènes Périodiques. Par M. Quetelet. 4to. 1855.

L'Académie Royale de la Belgique :—Annuaire. 1855. 8vo.

Bulletins des Séances de la Classe des Sciences, Année 1854. 8vo.

Mémoires de l'Académie des Sciences de l'Institut Impérial de France: Sciences Morales et Politiques. Tome IX. 4to. Paris, 1855.

Franklin Institute of Pennsylvania—Journal, Vol. XXIX, No. 6.; Vol. XXX. Nos. 1–3. 8vo. 1855.

Geographical Society, Royal—Address by the Earl of Ellesmere, May, 1855. 8vo.

Geological Society—Quarterly Journal, No. 43. 8vo. 1855.

Graham, George, Esq. Registrar-General—Weekly Report of the Registrar-General for July to October, 1855. 8vo.

Graty, Col. A. M. (the Author)—Mémoires sur les Productions Minérales de de la confédération Argentine. 16mo. Paris, 1855.

Highley, Mr. (the Publisher)—The Asylum Journal of Mental Science. No. 15. 8vo. 1855.

Hopkins, Evan, Esq. C.E. F.G.S. (the Author)—Illustrated Introduction to the Connection of Geology and Magnetism. 8vo. 1855.

Liverpool Literary and Philosophical Society — Proceedings, No. 9. 8vo. 1854–5.

Macloughlin, D. M.D. M.R.I. (the Author)—Two Letters on Cholera. 8vo. 1855.

Morris, H. S. M.D. M.R.I.—Laënnec, R. T. H.—Traité de l'Auscultation Médiate et des Maladies des Poumons et du Cœur. 2e. edition. 2 vols. 8vo. Paris, 1826.

Roche, L. Ch. & Sanson, L. J.—Nouveaux Elémens de Pathologie Médico-chirurgicale. 2e. edition. 5 vols. 8vo. Paris, 1828.

Richter, A. L.—Die Necrose. 8vo. Berlin, 1826.

Leupolt, J.—Grundzüge einer Propädeutik zum Studium der Heilkunde. 8vo. Berlin, 1826

Bartels, E. D. A.—Lehrbuch der allgemeinen Therapie. 8vo. Marburg, 1824.

Schmalz, K. G.—Versuch einer Medizinisch-chirurgischen Diagnostik, in Labellen. fol. Dresden, 1825.

Broussais, F. J. V.—De l'Irritation et de la Folie. 8vo. Paris, 1820.

Ennemoser, J.—über die Seele. 8vo. Bonn, 1824–5.

Newton, William, Esq. (the Author)—London, Westminster, and Southwark in the Olden Times, being a bird's-eye view of the City and its suburbs, in the reign of Henry VIII., with a Memoir. folio. 1855.

Novello, Mr. (the Publisher)—The Musical Times, for July to October, 1855. 4to.

Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie. Hefte 5, 6. 4to. Gotha, 1855.

Photographic Society—Journal, No. 32–35. 8vo. 1855.

Royal Society of Edinburgh—Proceedings, No. 45. 8vo. 1854–5.

Transactions, Vol. XXI. Part 2. 4to. 1854–5.

Royal Society of London—Philosophical Transactions, Vol. CXLV. Part 1. 4to. 1855.

Proceedings, Vol. VII. No. 14. 8vo. 1855.

St. Petersbourg, Académie Impériale de—Bulletin de la Classe Physico-Mathématique. Tome XIII. 4to. 1855.

Smithsonian Institution, Washington—Smithsonian Contributions to Knowledge. Vol. VII. 4to. 1855.

Eighth and Ninth Annual Reports. 8vo. 1854–5.

Society of Arts—Journal, for July to October, 1855. 8vo.

Sopwith, Thos. Esq. F.R.S. (the Author)—Addresses to the Tyneside Naturalists' Field Club, May 23, 1855, 9th Anniversary. 8vo. 1855.

Statistical Society—Journal, Vol. XVIII. Part 3. 8vo. 1855.

Taché, M. J. C. M.P. Canadien (the Author)—Esquisse sur le Canada. 16mo. 1855.

Taylor, Rev. W. F.R.S. M.R.I.—Magazine for the Blind. Nos. 13, 14. 4to. The Yorkshire Dialect exemplified. 8vo. 1839.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, März zu August, 1855. 4to. Berlin.
Visitors of the County Lunatic Asylum, at Hanwell—Ninth and Tenth Reports. 8vo. 1854-5.
Watkins, C. R. W. Esq. (the Author)—Principles and Rudiments of Mineralogy, Botany, Zoology. 12mo. 1855.
Webster, John, M.D. F.R.S. M.R.I.—Journal of Public Health, Part II. 8vo. 1855.
Winslow, Forbes, M.D. D.C.L. (the Author)—The case of Luigi Buranelli Medico-legally considered. 8vo. 1855.
Zoological Society of London—Proceedings, 1851-5. 8vo.

GENERAL MONTHLY MEETING,

Monday, December 3.

AARON ASHER GOLDSMID, Esq.,
in the Chair.

Delamore J. Bailey, Esq.,

was *admitted* a Member of the Royal Institution.

The Secretary reported that the following Arrangements had been made for the Lectures before Easter, 1856:—

Six Lectures on the DISTINCTIVE PROPERTIES OF THE COMMON METALS (adapted to a Juvenile Auditory), by MICHAEL FARADAY, Esq. D.C.L. F.R.S. &c. Fullerian Professor of Chemistry, R.I.

Twelve Lectures on PHYSIOLOGY AND COMPARATIVE ANATOMY, by THOMAS HENRY HUXLEY, Esq. F.R.S. Fullerian Professor of Physiology, R.I.

Eight Lectures on LIGHT, by JOHN TYNDALL, Esq. F.R.S. Professor of Natural Philosophy, R.I.

Eight Lectures on ORGANIC CHEMISTRY, by WILLIAM ODLING, Esq. M.B. Professor of Practical Chemistry at Guy's Hospital.

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM
Asiatic Society of Bengal—Journal, No. 250. 8vo. 1855.
Astronomical Society, Royal—Monthly Notices. Vol. XV. No. 9. 8vo. 1855.
Barlow, Rev. J. M.A. Sec. R.I.—Lord Alvanley on the State of Ireland. 8vo. 1841.

Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for Nov. 1855. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for Nov. 1855. 4to.
Bradbury, Henry, Esq. M.R.I.—The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Part 8. fol. 1855.
British Architects, Royal Institute of—Proceedings in Nov. 1855. 4to.
Civil Engineers, Institution of—Proceedings in Nov. 1855. 8vo.
College of Surgeons, Royal—Catalogue of the Histological Series in the Museum. Vol. II. 4to. 1855.
List of Members. 8vo. 1855.
Editors—The Medical Circular for Nov. 1855. 8vo.
The Practical Mechanic's Journal for Nov. 1855. 4to.
The Journal of Gas-Lighting for Nov. 1855. 4to.
The Mechanic's Magazine for Nov. 1855. 8vo.
The Athenæum for Nov. 1855. 4to.
Newton's London Journal for Nov. 1855. 8vo.
Franklin Institute of Pennsylvania—Journal, Vol. XXX. No. 4. 8vo. 1855.
Berichte, 1854. Hefte 1 and 2. 8vo.
Geological Society—Quarterly Journal, No. 44. 8vo. 1855.
Graham, George, Esq. Registrar-General—Report of the Registrar-General for Nov. 1855. 8vo.
Guthrie, G. J. Esq. F.R.S. (the Author).—Addenda to his Commentaries on Surgery. 16mo. 1855.
Hoper, Richard, Esq. M.R.I.—The Organ: its History and Construction. By E. J. Hopkins and E. F. Rimbault. 8vo. 1855.
Holland, Sir Henry, Bt. M.D. F.R.S. M.R.I. (the Author).—Medical Notes and Reflections. Third Edition. 8vo. 1855.
Medical and Chirurgical Society, Royal—Medico-Chirurgical Transactions, Vol. XXXVIII. 8vo. 1855.
North of England Institute of Mining Engineers—Transactions, Vol. III. 8vo. 1854-5.
Novello, Mr. (the Publisher)—The Musical Times for Nov. 1855. 4to.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie. Hefte 7, 8. 4to. Gotha, 1855.
Photographic Society—Journal, No. 36. 8vo. 1855.
Quaritch, Mr. B. (the Publisher)—A Practical Grammar of the Arabic Tongue, by Faris El-Shidiac. 18mo. 1856.
Royal Society.—Proceedings, Vol. VIII. No. 15. 8vo. 1855.
Robinson, Henry, Esq. C.E. (the Author)—A Plan for the effectual Improvement of the River Thames. 8vo. 1855.
Sächsische Gesellschaft, Königliche, Leipzig, Math.-Nat. Classe. Abhandlungen, Band IV. 3 Hefte. 8vo. 1855.
Society of Arts—Journal for Nov. 1855. 8vo.
Vincent, B. Assist. Sec. R.I. (the Editor)—Haydn's Dictionary of Dates. 7th edition. 8vo. 1855.
Ward, S. H. M.D. (the Author)—Healthy Respiration. 16mo. 1855.
Weale, John, Esq. (the Publisher)—Rudimentary Treatise on Galvanism, by Sir W. Snow Harris. 16mo. 1855.
Rudimentary Treatise on Railways, by E. D. Chattaway. 16mo. 1855.
Five Legislative Enactments for the Guidance of Contractors, Merchants, and Tradesmen. 16mo. 1855.

WEEKLY EVENING MEETING,

Friday, January 25, 1856.

SIR BENJAMIN COLLINS BRODIE, D.C.L. F.R.S. Vice-President, in the Chair.

W. R. GROVE, Esq. Q.C. F.R.S. M.R.I.

Inferences from the Negation of Perpetual Motion.

SCATTERED among the writings of philosophers will be found allusions to the subject of perpetual motion, and here and there are arguments like the following; such a phenomenon cannot take place, or such a theory must be fallacious, because it involves the idea of perpetual motion: thus Dr. Roget advanced as an argument against the contact theory of electricity, as originally propounded, that if mere contact of dissimilar metals, without any chemical or molecular change, could produce electricity, then as electricity could, in its turn, be made to produce motion, we should thus get perpetual motion.

It may be well to define, as far as such a definition is possible, what is commonly meant by the term perpetual motion. In one sense, all motion, or rather all force, is perpetual; for example, if a clock weight be wound up, it represents the force derived from the muscles of the arm which turns the key, the muscles again derive force indirectly from the chemical action of the food, and so on. As the weight descends it conveys motion to the wheels and pendulum; the former giving force off in the form of heat from friction, the latter communicating motion to the air in contact with it, thence to the case of the clock, thence to the air of the room,—proved in a very simple manner by the ticking heard, which is in fact, a blow to the organ of hearing. Although ultimately lost to our senses, there is no reason to suppose that the force is ever in fact lost. The weight thus acting, reaches the ground quietly, and produces no effect at the termination of its course.

If, instead of being allowed to communicate its force to the works of the clock, the weight be allowed to descend suddenly, as by cutting the string by which it is suspended, it strikes the floor with a force which shakes the house; and thus conveys, almost instantaneously, the amount of force which would be gradually dissipated, though not ultimately consumed, by the clock in a week or nine days.

This idea, however, of the perpetuity of force, is not what is

commonly understood by the term perpetual motion: that expression is used to convey the notion of a motive machine, the initial force of which is restored by the motion produced by itself,—a clock, so to speak, which winds itself up by its own wheels and pendulum, a pump which keeps itself going by the weight of the water which it has raised. Another notion, arising from a confusion between static and dynamic forces, was, that motion might be obtained without transferring force, as by a permanent magnet. All sound philosophers are of opinion that such effects are impossible; the work done by a given force, even assuming there were no such thing as friction, aerial resistance, &c., could never be more than equal to the initial force; the theoretical limit is equilibrium. The weight raised at one end of a lever can never, without the fresh application of extraneous force, raise the opposite weight which has produced its own elevation. A force can only produce motion when the resistance to it is less powerful than itself; if equal, it is equilibrium: thus if motion be produced, the resistance, being less than the initial or producing force, cannot reproduce this; for then the weaker would conquer the stronger force.

The object of this evening's communication was not, however, to adduce proofs that perpetual motion, in the sense above defined, is impossible; but assuming that as a recognised truth, to show certain consequences which had resulted, and others which were likely to result, from the negation of perpetual motion; and how this negation may be made a substantive and valuable aid to scientific investigation.

After Oersted made his discovery of electro-magnetism, philosophers of the highest attainments argued, that as a current of electricity, circulating in a wire round a bar of iron, produced magnetism, and as action and reaction are equal, and in contrary directions, a magnet placed within a spiral of wire should produce in the wire an electrical current: had it occurred to their minds that, if a permanent magnet could so produce electricity, and thence necessarily motion, they would thus get, in effect, perpetual motion, they would probably have anticipated the discovery of Faraday, and found that all that was required was to move the magnet with reference to the wire, and thus electricity might have been expected to be produced by a magnet without involving the supposed absurdity.

In a very different instance, viz. the expansion of water when freezing, not only heat, or the expansive force given to other bodies by a body cooling, would be given out by water freezing, but also the force due to the converse expansion in the body itself; and upon the argument that force would, in this case, be got out of nothing, Mr. J. Thomson saw that this supposed impossibility would not result if the freezing point of water were lowered by pressure, which was experimentally proved to be the case, by his brother.

In the effects of dilatation and contraction by heat and cold, when applied to produce mechanical effects, and consequently in the theory of the steam-engine, this subject possesses a greater practical interest. Watt supposed, that a given weight of water required the same quantity of what is termed total heat (that is, the sensible added to the latent heat) to keep it in the state of vapour, whatever was the pressure to which it was subjected, and consequently, however its expansive force varied. Clement Desormes was also supposed to have experimentally verified this law. If this were so, vapour raising a piston with a weight attached would produce mechanical power; and yet the same heat existing as at first, there would be no expenditure of the initial force; and if we suppose that the heat in the condenser was the real representative of the original heat, we should get perpetual motion. Southern supposed that the latent heat was constant, and that the heat of vapour under pressure increased as the sensible heat. M. Despretz, in 1832, made some experiments which led him to the conclusion that the increase was not in the same ratio as the sensible heat, but that yet there was an increase; a result confirmed and verified with great accuracy by M. Regnault, in some recent and elaborate researches. What seems to have occasioned the error in Watt and Clement Desormes' experiments was, the idea involved in the term latent heat; by which supposing the phenomenon of the disappearance of sensible heat to be due to the absorption of a material substance, that substance, 'caloric,' was thought to be restored when the vapour was condensed by water, even though the water was not subjected to pressure; but to estimate the total heat of vapour under pressure the vapour should be condensed while subjected to the same pressure as that under which it is generated, as was done in M. Despretz and M. Regnault's experiments.

Carnot's theory, that the mechanical force is produced by the transfer of heat, and that there is no ultimate cost or expenditure of heat in producing it, was founded in part on similar considerations; it is true that mechanical *motion* may be produced by the transfer of heat from a higher to a lower temperature, without ultimate loss, or, strictly speaking, with an infinitely small loss, but not, as he seemed to think, an available mechanical force, except upon an assumption which he did not make, and to which allusion will presently be made. Thus, let a weight be supposed to rest on a piston confining air of a certain temperature, say 50°, in a vessel non-conducting for heat; part of this temperature will be due to the pressure exerted, since compression produces heat in air, while dilatation produces cold. If the air be now heated, say to 70°, the piston, with the weight attached, will rise, and the temperature in consequence of the expansion of the air will cool somewhat, say to 69°, (the heat of friction of the piston may be taken to compensate the power lost by friction): if now a cold body be made to abstract 20°, the piston descending will, by its pressure, restore the 1° lost

by expansion; and when the piston has returned to its first position, the original 50° will remain as at first. Suppose this experiment repeated up to the rise of the piston; but when the piston is at its full elevation, and the cold body is applied, let the weight be removed, so as to drop upon a wheel, or to be used for other mechanical purposes, the descending piston will not now reach its original point without more heat being abstracted; from the removal of the weight there will not be the same force to restore the 1°, and the temperature will be 49°, or some fraction short of the original 50°; if this were otherwise, then as the ball in falling may be made to produce heat by friction, we should have more heat than at first, or a creation of heat out of nothing, in other words, perpetual motion.

When force is abstracted from a thermal machine we ought to lose heat, if we suppose degrees of heat at a lower temperature to represent the same amount of force as the same number of degrees at a higher temperature; if, for instance, we suppose that a body cooling from 120° to 100°, gives off the same force as a body cooling from 20° to zero; this seems to be tacitly assumed by Carnot, but is probably not correct, the results of high-pressure steam, and other facts indicating a contrary conclusion. If then the 20° on the lower scale do not represent an equivalent force to the 20° on the higher, we *may* gain the same heat in degrees in the condenser as was lost from the furnace, and yet get derived power. There is frequently a confusion between the work performed which returns to the machine, and the derived work, or that which does not return, and is used for other purposes. This is puzzling to the reader of treatises on the steam-engine, and kindred subjects, and has led to much obscurity of thought and expression.

M. Seguin, in 1839, controverted the position that derived power could be got by the mere transfer of heat, and by calculation from certain known data, such as the law of Mariotte, viz. that the elastic force of gases and vapours increased directly with the pressure, and assuming that for vapour between 100° and 150° centigrade each degree of elevation of temperature was produced by a thermal unit, he deduced the equivalent of mechanical work capable of being performed by a given decrement of heat; and thus concluded that for ordinary pressures about one gramme of water losing one degree centigrade would produce a force capable of raising a weight of 500 grammes through a space of one metre; this estimate is a little beyond that given by the more recent experiments of Mr. Joule. M. Seguin has, however, since the accurate and elaborate experiments of M. Regnault, necessarily varied his estimate, as by these experiments it appears that, within certain limits, for elevating the temperature of compressed vapour by one degree, no more than about $\frac{3}{10}$ths of a degree of total heat is required; consequently, the equivalent multiplied in this ratio would be 1666 grammes, instead of 500. Other investigators have given numbers more or less dis-

cordant; so that without giving any opinion on their different results, this question may be considered at present far from settled. M. Regnault himself does not give the law by which the ratio of heat varies with reference to the pressure, and is still believed to be engaged in researches on the subject, one involving questions of which experiments on the mechanical effects of elastic fluids seem to offer the most promising means of solution.

One of the greatest difficulties which had presented itself to Mr. Grove's mind, with reference to the theory of Carnot, had been one of analogy, derived from the received theories of electricity. Many electrical cases might be cited in which no electricity is supposed to be lost, though a certain mechanical effort is produced by the electricity; if, for instance, a ball vibrates between a positively and negatively electrified substance, none of our electrical theories lead us to believe that any difference in the actual amount of electricity transferred would be occasioned by the ball being attached to a lever which would strike a wheel or produce any other mechanical effect.

In preparing this evening's communication an experiment had occurred to him, which, though performed with imperfect apparatus and therefore requiring verification, does, as far as it goes, support the view derived from the negation of perpetual motion, viz. that when electricity performs any mechanical work which does not return to the machine, electrical power is lost. The experiment is made in the following manner. A Leyden jar of one square foot coated surface has its interior connected with a Cuthbertson's electrometer, between which and the outer coating of the jar are a pair of discharging balls fixed at a certain distance (about ½ an inch apart). Between the Leyden jar and the prime conductor is inserted a small unit jar of nine square inches surface, the knobs of which are 0·2 inch apart.

The balance of the electrometer is now fixed by a stiff wire inserted between the attracting knobs, and the Leyden jar charged by discharges from the unit jar. After a certain number of these, (22 in the experiment performed in the theatre on this occasion,) the discharge of the large jar takes place across the ½-inch interval; this may be viewed as the expression of electrical power received from the unit jar. The experiment is now repeated, the wire between the balls having been removed, and therefore the 'tip' or the raising of the weight, is performed by the electrical repulsion and attraction of the two pairs of balls; at 22 discharges of the unit jar the balance is subverted, and one knob drops upon the other, but *no discharge takes place*, showing that some electricity has been lost, or converted into the mechanical power which raises the balance. By another mode of expression the electricity may be supposed to be masked or analogous to latent heat, and would be restored if the ball were brought back, without discharge, by *extraneous force.*

This experiment has succeeded in so large an average of cases, and so responds to theory, that, notwithstanding the imperfection of the apparatus, Mr. Grove places much reliance on it; indeed, it is difficult to see, if the discharges or other electrical effects were the same in both cases, why the raising the ball, being extra, and the ball being capable by its fall of producing electricity or other force, force would not thus be got out of nothing, or perpetual motion attained.

The experiment is believed to be new, and to be suggestive of others of a similar character, which may be indefinitely varied. Thus, two balls made to diverge by electricity should not give to an electrometer the same amount of electricity as if they were, whilst electrified, kept forcibly together, an experiment which may be tried by Coulomb's torsion balance.

There is an advantage in electrical experiments of this class, as compared with those on heat, viz. that though there is no perfect insulation for electricity, yet our means of insulation are immeasurably superior to any attainable for heat.

Similar reasoning might be applied to other forces; and many cases, bearing on this subject, have been considered by Mr. Grove in his essay on the "Correlation of Physical Forces."

Certain objections to these views were then discussed, and especially some apparently formidable ones presented by M. Matteucci in a paper published by him some time ago.*

This distinguished philosopher cites the fact, that a voltaic battery decomposing water in a voltameter, while the same current is employed at the same time to make an electro-magnet, nevertheless gives in the voltameter an equivalent of gas, or decomposed substance, for each equivalent of chemical decomposition in the cells, and will give the same ratios if the electro-magnet be removed. In answer to this objection it may be said, that in the circumstances under which this experiment is ordinarily performed, several cells of the battery are used, and so there is a far greater amount of force generated in the cells than is indicated by the effect in the voltameter. If, moreover, the magnet is not interposed, still the magnetic force is equally existent through the whole circuit; for instance, the wires joining the plates will attract iron filings, deflect magnetic needles, &c. By the iron core a small portion of the force is absorbed while it is being made a magnet, but this ceases to be absorbed when the magnet is made; this is proved by the recent observations of Mr. Latimer Clarke, which were fully entered into and extended by Mr. Faraday, in a lecture at the Institution (Jan. 20, 1854†). It is like the case of a pulley and weight, which latter exhausts force while it is being raised, but when raised the force is free, and may be used for other purposes.

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* Archives des Sciences Physiques, Vol. IV., p. 380.

† Proceedings of the Royal Institution, Vol. I., p. 345.

If a battery of one cell, just capable of decomposing water and no more, be employed, this will cease to decompose while making a magnet. There must, in every case, be preponderating chemical affinity in the battery cells, either by the nature of its elements or by the reduplication of series, to effect decomposition in the voltameter, and if the point is just reached at which this is effected, and the power is then reduced by any resistance, decomposition ceases: were it otherwise, were the decomposition in the voltameter the exponent of the entire force of the generating cells, and these could independently produce magnetic force, this latter force would be got from nothing, and perpetual motion be obtained.

In another case, cited by M. Matteucci, viz. that a piece of zinc dissolved in dilute sulphuric acid gives somewhat less heat than when the zinc has a wire of platinum attached to it, and is dissolved by the same quantity of acid, the argument is deduced that as there is more electricity in the second than in the first case, there should be less heat; but, as according to our received theories the heat is a product of the electric current, and in consequence of the impurity of zinc, electricity is generated in the first case molecularly in what is called local action, though not thrown into a general direction, there should be more of both heat and electricity in the second than in the first case; as the heat and electricity due to the voltaic combination of zinc and platinum are added to that excited on the surface of the zinc, and the zinc should be, as in fact it is, more rapidly dissolved. Other instances are given by M. Matteucci, and many additional cases of a similar description might be suggested. But although it is difficult, perhaps impossible, to restrict the action of any one force to the production of one other force, and one only; yet if the whole of one force, say chemical action, be supposed to be employed in producing its full equivalent of another force, say heat; then as this heat is capable in its turn of reproducing chemical action, and, in the limit, a quantity equal to or at least only infinitesimally less than the initial force; if this could at the same time produce independently another force, say magnetism, we could, by adding this to the total heat, get more than the original chemical action, and thus create force or obtain perpetual motion.

The impossibility of perpetual motion thus becomes a valuable test of the approach that in any experiment we may have made to eliminating the whole power which a given natural force is capable of producing; it also serves, when any new natural phenomenon is discovered, to enable us to ascertain how far this can be brought into relation with those previously known. Thus when Moser discovered that dissimilar metals would impress each other respectively with a faint image of their superficial inequalities; that, for instance, a copper coin placed on a polished silver plate, even in the dark, would, after a short time, leave on the silver plate an impression of its own device, it occurred to Mr. Grove that as this experiment showed a

physical radiation taking place between the metals, it would afford a reason for the effects produced in Volta's contact experiment, without supposing a force without consumption or change in the matter evolving it. This led him to try the effect of closely approximating discs of zinc and copper without bringing them into metallic contact; and it was found that discs thus approximated, and then quickly separated, affected the electroscope just as though they had been brought into contact. Without giving any opinion as to what may be the nature of the radiation in Moser's phenomena, this experiment removes the difficulty presented by that of Volta to the chemical theory of electricity.

The general scope of the argument from the negation of perpetual motion leads the mind to regard the so-called imponderables as modes of motion, and not as different kinds or species of matter; the recent progress of science is continually tending to get rid of the hypotheses of fluids, of occult qualities, or latent entities, which might have been necessary in an earlier stage of scientific enquiry, and from which it is now extremely difficult to emancipate the mind: but if we can, as it is to be hoped we shall, ultimately arrive at a general dynamic theory, by which the known laws of motion of masses can be applied to molecules, or the minute structural parts of matter, it seems scarcely conceivable that the mind of man can further simplify the means of comprehending natural phenomena.

[W. R. G.]

WEEKLY EVENING MEETING,

Friday, February 1.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Professor Tyndall, F.R.S.

On the Disposition of Force in Paramagnetic and Diamagnetic Bodies.

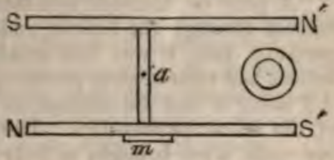
The notion of an attractive force, which draws bodies towards the centre of the earth, was entertained by Anaxagoras and his pupils, by Democritus, Pythagoras, and Epicurus; and the conjectures of these ancients were renewed by Galileo, Huyghens, and others, who stated that bodies attract each other as a magnet attracts iron.

Kepler applied the notion to bodies beyond the surface of the earth, and affirmed the extension of this force to the most distant stars. Thus it would appear, that in the attraction of iron by a magnet originated the conception of the force of gravitation. Nevertheless, if we look closely at the matter, it will be seen that the magnetic force possesses characters strikingly distinct from those of the force which holds the universe together. The theory of gravitation is, that every particle of matter attracts every other particle; in magnetism also we have the phenomenon of attraction, but we have also, at the same time, the fact of repulsion, and the final effect is always due to the difference of these two forces. A body may be intensely acted on by a magnet, and still no motion of translation will follow, if the repulsion be equal to the attraction. A dipping needle was exhibited: previous to magnetization, the needle, when its centre of gravity was supported, stood accurately level; but, after magnetization, one end of it was pulled towards the north pole of the earth. The needle, however, being suspended from the arm of a fine balance, it was shown that its *weight* was unaltered by its magnetization. In like manner, when the needle was permitted to float upon a liquid, and thus to follow the attraction of the north magnetic pole of the earth, there was no motion of the mass towards the pole referred to; and the reason was known to be, that although the marked end of the needle was *attracted* by the north pole, the unmarked end was *repelled* by an equal quantity, and these two equal and opposite forces neutralized each other as regards the production of a motion of translation. When the pole of an ordinary magnet was brought to act upon the swimming needle, the latter was attracted,—the reason being that the attracted end of the needle being much nearer to the pole of the magnet than the repelled end, the force of attraction was the more powerful of the two; but in the case of the earth, the pole being so distant, the length of the needle was practically zero. In like manner, when a piece of iron is presented to a magnet, the nearer parts are attracted, while the more distant parts are repelled; and because the attracted portions are nearer to the magnet than the repelled ones, we have a balance in favour of attraction. Here then is the most wonderful characteristic of the magnetic force, which distinguishes it from that of gravitation. The latter is a simple unpolar force, while the former is duplex or polar. Were gravitation like magnetism, a stone would no more fall to the ground than a piece of iron towards the north magnetic pole: and thus, however rich in consequences the supposition of Kepler and others may have been, it was clear that a force like that of magnetism would not be able to transact the business of the universe.

The object of the evening's discourse was to inquire whether the force of diamagnetism, which manifested itself as a repulsion of certain bodies by the poles of a magnet, was to be ranged as a polar force, beside that of magnetism; or as an unpolar force, beside that of *gravitation*. When a cylinder of soft iron is placed within a helix,

and surrounded by an electric current, the antithesis of its two ends, or in other words, its polar excitation, is at once manifested by its action upon a magnetic needle; and it may be asked why a cylinder of bismuth may not be substituted for the cylinder of iron, and its state similarly examined. The reason is, that the excitement of the bismuth is so feeble, that it would be quite masked by that of the helix in which it is enclosed; and the problem that now meets us is, so to excite a diamagnetic body that the pure action of the body upon a magnetic needle may be observed, unmixed with the action of the body used to excite the diamagnetic.

How this may be effected, was illustrated in the following manner:—an upright helix of covered copper wire was placed upon the table, and it was shown that the top of the helix attracted, while its bottom repelled the same pole of a magnetic needle; its central point, on the contrary, was neutral, and exhibited neither attraction nor repulsion. This helix was caused to stand between the two poles N′ S′ of an astatic magnet; the two magnets S N′ and S′N were united by a rigid cross piece at their centres, and suspended from the point *a*, so that both magnets swung in the same horizontal plane. It was so arranged that the poles N′S′ were opposite to the central or neutral point of the helix, so that when a current was sent through the latter, the magnets were unaffected by the current. Here then we had an excited helix which itself had no action upon the magnets, and we were thus at liberty to examine the action of a body placed within the helix and excited by it, undisturbed by the influence of the latter. The helix was 12 inches high, and a cylinder of soft iron 6 inches long suspended from a string and passing over a pulley could be raised or lowered within the helix. When it was so far sunk that its lower end rested upon the table, the upper end found itself between the poles N S attracting one of them, and repelling the other, and consequently deflecting the astatic system in a certain direction. When the cylinder was raised so that the upper end was at the level of the top of the helix, its lower end was between the poles N′ S′; and a deflection opposed in direction to the former one was the immediate consequence. To render these deflections more visible to the audience, a mirror *m*, was attached to the system of magnets; a beam of light thrown upon the mirror was reflected and projected as a bright disk against the wall of the theatre; the distance of this image from the mirror being considerable, and its angular motion double that of the latter, a very slight motion of the magnet was sufficient to produce a displacement of the image through several yards. This then is the principle of the beautiful apparatus* by which the investigation now



* Devised by *Prof. W. Weber*, and constructed by M. Leyser, of Leipzig.

brought forward was conducted. It is manifest that if a second helix be placed between the poles S N with a cylinder within it, the action upon the astatic magnet may be exalted. This was the arrangement made use of in the actual inquiry. Thus to intensify the feeble action, which it is here our object to seek, we have in the first place neutralized the action of the earth upon the magnets, by placing them astatically. Secondly, by making use of two cylinders, and permitting them to act simultaneously on the four poles of the magnets, we have rendered the deflecting force four times what it would be, supposing only a single pole to be used. Finally, the whole apparatus was enclosed in a suitable case, which protected the magnets from atmospheric currents, and the deflections were read off through a glass plate in the case, by means of a telescope and scale placed at a considerable distance from the instrument.

A pair of bismuth cylinders was first examined. Sending a current through the helices, and observing that the magnets swung perfectly free, it was first arranged that the cylinders within the helices had their central points opposite to the poles of the magnets. All being at rest the number on the scale marked by the cross wire of the telescope was 572. The cylinders were then moved so that two ends were brought to bear simultaneously upon the magnetic poles: the magnet moved promptly, and after some oscillations* came to rest at the number 612; thus moving from a smaller to a larger number. The other two ends of the bars were next brought to bear upon the magnet: a prompt deflection was the consequence, and the final position of equilibrium was 526; the movement being from a larger to a smaller number. We thus observe a manifest polar action of the bismuth cylinders upon the magnet; one pair of ends deflecting it in one direction, and the other pair deflecting it in the opposite direction.

Substituting for the cylinders of bismuth thin cylinders of iron, of magnetic slate, of sulphate of iron, carbonate of iron, protochloride of iron, red ferrocyanide of potassium, and other magnetic bodies, it was found that when the position of the magnetic cylinders was the same as that of the cylinders of bismuth, the deflection produced by the former was always opposed in direction to that produced by the latter; and hence the disposition of the force in the diamagnetic body must have been precisely antithetical to its disposition in the magnetic ones.

But it will be urged, and indeed has been urged against this inference, that the deflection produced by the bismuth cylinders is purely due to the currents of induction excited in the mass by its motion within the helices. In reply to this objection, it may be stated, in the first place, that the deflection is permanent, and cannot therefore be due to induced currents, which are only of momentary duration. It has also been urged that such experiments ought to be

* *To lessen these a copper damper was made use of.*

made with other metals, and with better conductors than bismuth, for if due to currents of induction the better the conductor the more exalted will be the effect. This requirement was complied with.

Cylinders of antimony were substituted for those of bismuth. This metal is a better conductor of electricity, but less strongly diamagnetic than bismuth. If therefore the action referred to be due to induced currents we ought to have it greater in the case of antimony than with bismuth; but if it springs from a true diamagnetic polarity, the action of the bismuth ought to exceed that of the antimony. Experiment proves that the latter is the case, and that hence the deflection produced by these metals is due to their diamagnetic, and not to their conductive capacity. Copper cylinders were next examined: here we have a metal which conducts electricity fifty times better than bismuth, but its diamagnetic power is nearly null; if the effects be due to induction we ought to have them here in an enormously exaggerated degree, but no sensible deflection was produced by the two cylinders of copper.

It has also been proposed by the opponents of diamagnetic polarity to coat fragments of bismuth with some insulating substance, so as to render the formation of induced currents impossible, and to test the question with cylinders of these fragments. This requirement was also fulfilled. It is only necessary to reduce the bismuth to powder and expose it for a short time to the air to cause the particles to become so far oxidised as to render them perfectly insulating. The power of the powder in this respect was exhibited experimentally in the lecture; nevertheless, this powder, enclosed in glass tubes, exhibited an action scarcely less powerful than that of the massive cylinders.

But the most rigid proof, a proof admitted to be conclusive by those who have denied the antithesis of magnetism and diamagnetism, remains to be stated. Prisms of the same heavy glass as that with which the diamagnetic force was discovered, were substituted for the metallic cylinders, and their action upon the magnet was proved to be precisely the same in kind as that of the cylinders of bismuth. The inquiry was also extended to other insulators: to phosphorus, sulphur, nitre, calcareous spar, statuary marble, with the same invariable result: each of these substances was proved polar, the disposition of the force being the same as that of bismuth and the reverse of that of iron. When a bar of iron is set erect, its lower end is known to be a north pole, and its upper end a south pole, in virtue of the earth's induction. A marble statue, on the contrary, has its feet a south pole, and its head a north pole, and there is no doubt that the same remark applies to its living archetype; each man walking over the earth's surface is a true diamagnet, with its poles the reverse of those of a mass of magnetic matter of the same shape and in a similar position.

An experiment of practical value, as affording a ready estimate of the different conductive powers of two metals for electricity, was

exhibited, for the purpose of proving experimentally some of the assertions made by the speaker in reference to this subject. A cube of bismuth was taken and suspended by a twisted string between the two poles of an electro-magnet. The cube was attached by a short copper wire to a little square pyramid, the base of which was horizontal, and its sides formed of four small triangular pieces of looking-glass. A beam of light was suffered to fall upon this reflector, and as the reflector followed the motion of the cube the images cast from its sides followed each other in succession, each describing a circle of about 30 feet in diameter. As the velocity of rotation augmented, these images blended into a continuous ring of light. At a particular instant the electro-magnet was excited, currents were evolved in the rotating cube, and the strength of these currents, which increases with the conductivity of the cube for electricity, was practically estimated by the time required to bring the cube and its associated mirrors to a state of rest. With bismuth this time amounted to a score of seconds or more: a cube of copper, on the contrary, was struck almost instantly motionless when the circuit was established.

[J. T.]

GENERAL MONTHLY MEETING,

Monday, February 4.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

George Busk, Esq. F.R.S. F.R.C.S.
Major-General Anthony Emmett, Royal Engineers, and
William Baker Taylor, Esq. Surgeon-General, Bombay Army,

were duly *elected* Members of the Royal Institution.

Thomas Farquhar Hill, Esq. and
Jonathan Rigg, Esq.

were *admitted* Members of the Royal Institution.

The following Presents were announced, and the thanks of the Members were returned for the same:—

From—

H.R.H. Prince Albert, *K.G. F.R.S. Vice-Patron, R.I.*—W. Macgillivray, Natural History of Dee-Side and Braemar. Edited by E. Lankester, M.D. 8vo. 1855.

British Museum, Trustees of the—Catalogue of the Maps, Prints and Drawings, &c., forming the Geographical Collection attached to King George III.'s Library. 2 vols. 8vo. 1829.

Catalogues of the Zoological Collections. 36 Parts. 12mo. 1853–5.

Actuaries, Institute of—The Assurance Magazine. No. 22. 8vo. 1856.

Arnold, T. J. Esq. Life-Sub. R.I.—G. D. Barber; Suggestions on the Ancient Britons. 8vo. 1854.

Ancient Oral Records of the Cimri or Britons, in Asia and Europe, recovered. 16mo. 1855.

Asiatic Society of Bengal—Journal, No. 250. 8vo. 1855.

Astronomical Society, Royal—Monthly Notices. Vol. XVI. Nos. 1, 2. 8vo. 1855–6.

Basel, Naturforschende Gesellschaft—Verhandlungen, Zweite Heft. 8vo. 1855.

Bayley, F. Esq. M.R.I.—Bp. White Kennett, on Ecclesiastical Synods and Parliamentary Convocations in the Church of England. 8vo. 1701.

Letters on Bishop Merks; and on the Nomination, Election, Investiture, and Deprivation of English Prelates. 8vo. 1713–17.

Two Sermons: (on Gunpowder Treason, and on the Office of a Bishop.) 4to. 1706–15.

Abp. Wake, Sermon on Popery. 4to. 1706.

Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for Jan. Feb. 1856. 8vo.

Boosey, Messrs. (the Publishers)—The Musical World for Jan. Feb. 1856. 4to.

Bradbury, Henry, Esq. M.R.I.—The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Parts 9, 10. fol. 1855–6.

British Architects, Royal Institute of—Proceedings in Jan. 1856. 4to.

Chemical Society—Quarterly Journal, No. 32. 8vo. 1856.

Civil Engineers, Institute of—Proceedings in Jan. 1856. 8vo.

Dawes, R. M.A. Dean of Hereford (the Author)—Address at the Huddersfield Institute, Dec. 13, 1855. 16mo. 1856.

Editors—The Medical Circular for Jan. 1856. 4to.

The Practical Mechanic's Journal for Jan. 1856. 4to.

The Journal of Gas-Lighting for Jan. 1856. 4to.

The Mechanic's Magazine for Jan. 1856. 8vo.

The Athenæum for Jan. 1856. 4to.

The Engineer for Jan. 1856. 4to.

Faraday, Professor, D.C.L. F.R.S.—Monatsberichte der Königl. Preuss. Akademie, Sept.-Nov. 1855. 8vo. Berlin.

Appendix to Report of the Committee for Scientific Inquiries in relation to the Cholera Epidemic of 1854. 8vo. 1855.

Franklin Institute of Pennsylvania—Journal, Vol. XXX. Nos. 5, 6. 8vo. 1855.

Graham, George, Esq. (Registrar-General)—Report of the Registrar-General for Jan. 1856. 8vo.

Graves, Messrs. and Co. (the Publishers)—View of the Docks of Sebastopol. Nov. 1855.

Holland, Lady (the Author)—Memoir of the Rev. Sydney Smith; by his daughter Lady Holland; with a Selection from his Letters. Edited by Mrs. Austin. 3rd Edition. 2 vols. 8vo. 1855.

Irish Academy, Royal—Memoirs, Vol. XXII. Part 6. 4to. 1855.

Proceedings, Vol. VI. Part 2. 8vo. 1854–5.

Johnson, Edmund C. M.D. M.R.I. (the Author)—Inquiry into the Musical Instruction of the Blind in France, Spain, and America. 8vo. 1855.

Lewin, Malcolm, Esq. M.R.I. (the Author)—Speech (on Indian Government), Dec. 13, 1855. 8vo. 1856.

Torture in Madras. 8vo. 1855.

Linnean Society of London—Transactions, Vol. XXI. Part 4. 4to. 1855.

Proceedings, Nos. 59–66. 8vo. 1854–5.

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WEEKLY EVENING MEETING,

Friday, February 8.

Sir Henry Holland, Bart. M.D. F.R.S. Vice-President, in the Chair.

Professor Henry D. Rogers,
(From the United States,)

On the Geology and Physical Geography of North America.

The speaker stated that his object in this discourse was to present a condensed view of the physical features and geological outlines of North America,—a country highly interesting, from the majestic scale of its natural structure—from its peculiar position on the earth's surface, lying intermediate between Europe on the one hand, and Asia and Australasia on the other—as a half-way house to connect the commerce and civilization of the world; and tenanted by an energetic people, essentially a composite nation, made up of the enterprising and ardent spirits, who have gone from the various more civilized countries of Europe.

Taking a comprehensive but rapid survey of the general physical features of North America, we find it presents but two great slopes, one sinking towards the Atlantic, the other towards the Pacific, divided by a lofty mountain axis, the Chippewayan or Rocky Mountain chain, the true watershed or backbone of the continent.

From either base of this chain, where the plain of the country has an elevation of 5000 to 6000 feet above the sea, the continent slopes eastward and westward, interrupted by only two important intervening mountain swells—the Atlantic or Appalachian chain, and the Pacific or Californian. These oceanic chains only partially turn the drainage; some of the largest rivers, the Susquehanna and Kanawha, in the one case, Frazer's River and the Great Columbia in the other, cutting quite through them. Another broad swell of the surface crosses the continent, nearly westward, from Labrador to the sources of the Columbia, passing between the Lawrentian Lakes and Hudson's Bay, and between the sources of the Missouri and those of the Saskatchawan. From this the continent slopes gently northwards, to its Arctic shores, and southwards, to its Mexican. All these slopes are well indicated in the river drainage.

Looking closer, there appear seven grand primary divisions of

the surface of the continent, comprising two broad continental plains, three great belts of mountains, and two narrow oceanic slopes.

A.—The largest of these areas is the GREAT CENTRAL PLAIN, extending from the Gulf of Mexico to the Arctic Sea and Hudson's Bay, and from the Appalachians to the Rocky Mountains, a wide continental region of table-lands, plains, and gentle slopes.

It consists of two physically different districts—the one a western region of elevated table lands, or steppes; the other, a parallel eastern zone of less average elevation, comprising three great basins or plains, of river and lacustrine drainage.

I. *The Western Steppes.*—These constitute a wide zone, east of the Rocky Mountains, about 350 miles broad, ranging from the 28th to beyond the 60th degree of latitude; including, indeed, the Arctic Highlands, east of Mackenzie River. From the Arkansas to the Upper Missouri this plateau has a mean level above the sea of about 4000 feet, rising, at the base of the mountains, to 5000, and at the highest east and west swell of the surface, to 6000 feet. The general eastern boundary of this great terrace, which has a mean height of about 2000 feet, follows approximately the meridian of 98° W.; but northward of the Nebraska and Missouri, it is not so well defined. Rising to the foot of the Rocky Mountains, by successive steps and gentle slopes, the plain contains one escarpment, which being much more conspicuous than the rest, seems to divide it into two belts. This, which coincides with the meridian of 101° W., is best marked between lat. 32° and the Arkansas, but extends to the Forks of the Nebraska, and yet further. From the Pecos to the Nebraska, the upper plateau is about 3500 feet above the sea, and is 1000 feet higher than the table land east of it. This entire western belt, as far north as the Missouri, is a barren and thirsty treeless desert, especially in its southern half. It is without verdure, except along the very attenuated streams, and at certain seasons is entirely rainless. North of the Missouri it is better irrigated, and more grassy, and includes many rivers and lakes.

The lower terrace resembles the upper in its flat monotonous surface, its treeless wastes, and arid summer climates, but it is rather more grassy, and its southern streams are better fringed with trees and verdure. All the rivers, from the Pecos to the Arkansas, issue from the upper table land to the lower, through deep narrow sluices (cañons), between precipices of enormous height. For more than 400 miles, from the Brazos to the Arkansas, the lower treeless plain is fringed by a remarkable belt of woodland, from 5 to 25 miles wide, which is called the "Cross Timbers." This strip of forest forms the western boundary of the fertile and better watered still lower plain of Texas, a region of verdant prairies, dotted with natural parks, and clumps of live oaks and other noble trees.

II. *The Eastern Basins.*—This vast tract, embraced between *the north-west* base of the Appalachian mountains and the eastern

border of the elevated western plateau, is a broad well-watered plain, full of streams and lakes, rising nowhere higher than 1500 feet above the sea; and in few districts higher than 500 or 700 feet. It includes three large natural basins of drainage—that of the Mississippi, and its affluents east of the high western steppes; that of the Lawrentian Lakes and their feeders; and that of the southern, western and eastern tributaries of Hudson Bay.

1. *The Mississippi Basin.*—This wide river basin slopes so gently southward that its height at the mouth of the Missouri, nearly 700 miles from the sea, is only 388 feet; and at the Falls of St. Anthony, 1150 miles from the sea, it is no more than 856 feet. Its eastern and western slopes are likewise extremely gentle, as shown by the fact, that the elevation of the plain at Pittsburg, nearly 600 miles from the Mississippi, eastward, is only 679 feet; and in the opposite direction, at the mouth of the republican fork of the Kansas, it is still but 927 feet. The basin of the Mississippi and Missouri is divided from that of Hudson Bay,—from the western end of Lake Superior to the foot of the Rocky Mountains, but eastward from the drainage of the Lawrentian Lakes,—by a low watershed, nowhere higher than 1300 feet, and in few districts more than 1000 feet above the sea. This whole Mississippi plain enjoys a soil and climate of rare fertility, and, possessing enormous coal-fields and other mineral wealth, is endowed with extraordinary agricultural, commercial, and manufacturing resources. From the western slopes of the Appalachians to the Mississippi, the Wabash, and Lake Michigan, nearly the entire surface was originally clothed with forest; but west of those limits, the vast plain consists of gently-rolling verdant prairies, intersected by strips of woodland along the valleys of the streams; and on the south-west, sprinkled with parks, and scattered clumps of trees.

2. *The Lawrentian Lake Basin.*—This middle basin, from the head of Lake Superior to Lake Ontario and the Ottawa, is separated on its south from the basin of the Mississippi and Ohio by the low watershed bordering its lakes; and on the north, from the southern feeders of Hudson Bay by a somewhat higher watershed, ranging from Missabay Heights, where the summit tract is about 1500 feet high, north of Lake Superior and the Ottowa, and gradually declining to the sources of the Saguenay. It is a curious feature of this basin, that its western rim is only 50 miles west of the head of Lake Superior, the southern and northern watersheds which embrace it, uniting in about long. 92° 50′. An unusual portion of the surface of this basin is covered with water. Its five great lakes contrast their magnitude with the smallness of the area which supplies them; for both the watersheds are in relatively close proximity to these basins. In some districts the southern watershed is only a few miles distant from Lakes Michigan and Erie; while some of the streams descending southward are more than 2000 miles from the Gulf of Mexico. But this anomaly vanishes, when we recognise in these

lakes only expansions of the Upper St. Lawrence, their southern watershed then taking its true half-way position below the Gulf of St. Lawrence and the Gulf of Mexico. The basin of the five lakes is a wide level plateau falling eastward, by successive stages, from a level at the surface of Lake Superior of 628 feet, to one of 232 feet at the surface of Lake Ontario. Its confining watersheds are only the eastern extensions of the great central swell of the Continent, forking where it approaches Lake Superior; one branch ranging northward, the other southward, of this long and narrow basin.

3. *The Hudson Bay Basin.*—This almost rivals in size that of the Mississippi. It is separated from that of the St. Lawrence by a low, crescent-shaped, curving watershed, beginning in the highlands of Labrador and the Watchish Mountains, and extending westward in the Missabay Heights. To the south-west and west its boundary is the vaguely-traced limit of the western plateau, prolonged north-north-west from the Missouri towards Lake Athabaska. This great basin is a region of innumerable tortuous streams and inosculating lakes and swamps, converging their drainage into Hudson Bay. In the northern half of the continent, the larger rivers which enter Hudson Bay, as the Great Saskatchawan, descend from the base of the Rocky Mountains, across the low watershed which forms the western rim of this basin; thus linking it hydrographically with the high western plateau, and establishing, in a wider sense, one great hydrographic basin from the Watchish to the Rocky Mountains, north of the east and west swell of the continent. The general elevation of the extensive rim of the Hudson Bay Basin is scarcely anywhere higher than 1600 feet above the sea; the average level of the whole girdle probably not exceeding 1000 feet. It is a cold, inhospitable, and snowy district, its rocks deeply overlaid with sterile drift, and its surface extensively covered with a network of waters and wet swamps.

B. The Appalachian Mountain Zone.

This Atlantic mountain range embraces the whole belt of mountain ridges extending from the Gulf of St. Lawrence at Gaspé, to Georgia and Northern Alabama. It consists of two divisions—a north-eastern, ranging from Gaspé to the valley of the Hudson; and a south-western, thence to the interior of Alabama: the total length of the chain is about 1500 miles, while its breadth varies from 150 to 200 miles. In the chief divisions of its length it is a wide complex belt of many parallel ridges, whose crests nowhere ascend much beyond 4000 feet above the sea; the base of the chain, where highest, being about 1800 feet above the sea level. From Alabama to the Hudson, the Appalachian chain is the watershed between the Atlantic rivers and those of the Ohio and Lake Ontario; and from the Hudson to the Gulf of St. Lawrence it separates the drainage of those flowing to the Atlantic from the shorter streams entering the St. Lawrence. The Hudson valley

does not entirely separate this long, nearly continuous mountain system. A depression of the continent to a depth of only 300 feet would let the ocean through from the Gulf of St. Lawrence to the Bay of New York, and interpose a strait nearly as broad as that between England and the European continent, between the main land of North America and the even now half-insulated district of New England, New Brunswick, and Nova Scotia. That this great transverse depression of the surface, now uniting the estuaries of the St. Lawrence and the Hudson, was once thus actually submerged is proved by indisputable geological monuments, consisting of oceanic clays containing marine pleistocene tertiary fossils.

The *North-eastern* section of the Appalachians contains as its principal chain the Green Mountains, a long belt of parallel and swelling ridges commencing in the Nôtre Dame range south of the St. Lawrence, in Lower Canada, and terminating at the Schuylkill River in Pennsylvania. These mountains at the Hudson, under the name of the Highlands, are cut to their base by that tidal river. In Vermont their rounded summits are 4000 feet high, but on the Hudson they nowhere exceed 1600 feet. The White Mountains of New Hampshire, and the Adirondac group in the north-east corner of New York, are lofty outlying masses, subsidiary to the Appalachians. They consist chiefly of the crystalline rocks. Their highest peaks exceed 5000 feet, and that of Mount Washington, the loftiest east of the Rocky Mountains, is 6428 feet above the sea level.

South-western Appalachians.—From the Hudson and Mohawk to middle Alabama extends the other great division of the Appalachian chain, marked by an extraordinary parallelism, length, narrowness, steepness, and evenness of its numerous ridges. Its south-eastern belt, the most undulating and picturesque, is the Blue Ridge of Virginia, and the Smoky or Unaka Mountain range of Carolina, Georgia, and Tennessee; it is nearly identical in geological structure with the Green Mountains, and resembles them in contour and scenery. Its highest crests, which are in North Carolina and Tennessee, reach to 4000 and 5000 feet above the sea. The rocks of the Blue Ridge and Green Mountain chains are chiefly ancient metamorphic strata, as gneiss, talcoze, and chloritic schists, and highly altered argillaceous and sandy strata low in the palæozoic system, including, indeed, some of the oldest fossiliferous formations: these stratified masses are penetrated by igneous veins and dykes.

To the north-west of the Blue Ridge, and separated from it by a long continuous plain called the Great Appalachian Valley, runs the parallel range of the middle Appalachian belt, traceable from the St. Lawrence to Alabama. This central zone of mountains consists of long narrow level ridges, divided by narrow longitudinal valleys, and cut to their bases by sharp deep ravines permitting the passage of the rivers. These ridges, caused by excessive erosion of the parallel waves of the strata of which the central Appalachians consist, are linked into groups of several parallel crests, some of

them remarkably straight for great distances, others gently curving. In many instances two narrow mountain crests unite at the two extremities to inclose a deep trough-shaped valley; some of these valleys, with their mountain borders, having a remarkable resemblance to a long narrow canoe or skiff. One class have a synclinal structure, the highest strata being in the bed or centre of the valley, and the hard lower rocks forming the enclosing parallel mountain crests. The other class are anticlinal in their form, a powerful erosive action of waters having carved them out by cutting through the crests of the original crust-waves, and scooping down to the softer lower strata. In some portions of this middle belt the long narrow ridges unite in wider mountain table lands of the general height of the chain.

The rocks of this long middle Appalachian belt are exclusively palæozoic strata, comprising all from the base of that system to the upper coal measures inclusive. Scarcely a single igneous dyke or mineral vein intrudes itself into this, or the next more western division of the Appalachians.

The north-western belt of the Appalachians is a long and comparatively narrow high table land, called the Alleghany Mountain in Pennsylvania, the Sewell Mountain in Virginia, and the Cumberland Mountain in Tennessee. Almost the whole way continuously from North-eastern Pennsylvania to Northern Alabama, it presents a high escarped slope, facing south-eastward toward the middle chain. This table land is composed of nearly horizontal strata of the upper Devonian formations at its base, and of carboniferous rocks in its higher parts, wide tracts of it being covered exclusively by bituminous coal measures. Along its south-eastern border it is gently undulated with parallel broad regular anticlinal and synclinal flexures of the strata, the expiring waves of the middle Appalachian belt, where these crests are stronger, steeper, and more crowded. The plateau subsides to the north-west with the dying out of these flexures, and sinks into the broad plain of the basins of the Ohio and Mississippi.

A longitudinal survey of the Appalachian chain shows much undulation in its direction, and discovers ten distinct sections as respects its trend. Five of these are straight, three of them convex towards the north-west, and two of them convex towards the south-east.

C. The Rocky Mountains, or Chippewayan Chain.

This very elevated mountain axis, or main water-shed of the continent, consists generally of two, but in some districts of three, principal ranges, each composed of many high mountain crests, deep valleys, and elevated table lands. The great component ranges are approximately parallel, but they are variously linked and inosculated by transverse ridges. Their crest lines undulate

on a stupendous scale, and many of their summits are sharply notched and serrated like the Alps. The main eastern range rises like a huge rampart above the great table land at its base, presenting a deeply gashed flank and vast mountain buttresses towards the plain. Longitudinally the whole chain consists of many sections broken by deep passes, and by the cessation of the leading ridges, which, in many instances, do not join, but lap past each other. This recently ascertained feature presents unexpected facilities for the construction of railways, and other avenues of communication across the continent. In Northern Mexico the eastern range is the Cordillera of Cohahuela and Potozi, the Guadaloupe Mountains being only an outlying chain of ridges; and the western range is the Sierra de los Mimbres. Opposite the sources of the Arkansas, the eastern belt is the Moro and Chowatche, or Wet Mountain; and the western the San Juan, and La Plata Mountains. The noble valley of the Rio del Norte is here embraced between the two ranges. From the Arkansas to the north fork of the Platte, the chain is more complex and triple; its eastern range, comprising the Medicine Mountains, contains some of the loftiest peaks of the Rocky Mountains: such are the Spanish Peaks, Pike's, Long's, and Laramie's Peaks, all rising to 10,000 and 12,000 feet above the sea. North of the Platte rise the Wind River Mountains, the loftiest division of the chain. Here Fremont's Peak has an elevation of 13,568 feet. This high mountain axis is the focal watershed of the continent, for it parts the head fountains of the Missouri flowing to the Atlantic, from those of the Columbia and Rio Colorado, descending westward to the Pacific.

Branching southward from the Rocky Mountains, near the Wind River range, is the long and lofty chain called the Wahsatch. It is the eastern boundary of the Great Utah Desert, and the western barrier of the basin of the Rio Coloradò, the Rocky Mountains being the eastern. Northward of the Wind River chain the eastern, or main axis of the Rocky Mountains, is exceedingly high, where it divides the middle and northern streams of the Columbia from the sources of the Missouri and Saskatchawan. Near the head of the latter great river Mount Hooker towers to the height of 15,700 feet above the sea; and a little further north, Mount Brown, the feeder of the river Athabasca, reaches the yet greater altitude of 15,990 feet. From this culminating district the chain gradually declines northward to the Arctic Ocean. Beyond lat. 55° the main eastern crest ceases to be the watershed between the Pacific and Hudson Bay or the Arctic Ocean.

The geological constitution and structure of the Rocky Mountains are very imperfectly known. Granite and gneissic rocks, and various palæozoic strata, including much carboniferous limestone, have, however, been extensively noticed in many districts. Between the higher ridges occur table lands and valleys, consisting apparently of cretaceous and tertiary deposits.

D. The Great Western Desert Plateau.

Between the Rocky Mountains on the east, and the Pacific Alps, Cascade Chain, and Sierra Nevada on the west, there stretches from the Gulf of California to the Arctic Ocean, an elevated desert zone of great breadth and height, especially in its central section, between latitudes 35° and 45°, where its mean level above the sea is about 5000 feet. It consists of three principal regions,—the Central Desert Plateau, now indicated; a semi-desert area, south-east and south of this, the basin of the Rio Colorado, divided from it by the Wahsatch Mountains; and a northern region, that of the Columbia and Frazer's Rivers, sterile and rugged, separated from the central by the mountains south of the Columbia.

The Central Plateau, or Great Utah Desert, is an almost rainless region, parched by dry winds. The evaporation balancing the rain-fall, none of its scanty drainage passes to the sea, but each attenuated stream ends in a closed lake, the water of which is more or less salt. It is a vast elevated arid waste, containing wide plains encrusted with salt, a soil generally more or less saline, and large tracts of light volcanic scoriæ and ashes. Through its centre there runs, southward, a broad belt of straight mountain ridges, the Humboldt Mountains, composed in part of crystalline rocks, in part of carboniferous limestone, and other palæozoic strata. The chief stream is the Humboldt River, which, flowing west, dies away in the salt plain before it reaches the foot of the Sierra Nevada. There are twelve or fifteen salt lakes of considerable magnitude in this high insulated basin. The largest of these is the Great Mormon, or Utah Salt Lake, the waters of which are impregnated almost to saturation, containing 20 per cent. of common salt, and 2 per cent. only of other salts. Its length is 75 miles, and mean breadth about 30 miles.

The Southern Desert Basin, or that of the Rio Colorado, lying between the Rocky Mountains and the Wahsatch and Californian Mountains, is, like the Utah Desert, a succession of arid table lands and plains, intersected by rugged mountains. Its north-eastern portion is better fed with rain than its western; those tracts most under the lee of the intercepting barrier of the Pacific Mountains being more desiccated than the districts further removed. The Pacific Mountains are also less elevated in this latitude than further north, opposite the Central Desert. This Southern or Colorado Desert slopes gently southward to the ocean level at the head of the Gulf of California, north of which there is an arid tract of the surface, which is actually lower than the surface of the sea, believed to have a depression in its centre of 300 feet,—a region of continental depression, from which the dry winds have lapped up the remnant waters of the Gulf of California, and annually drink away every trace of the back waters of the Rio Colorado, at the season of its rise from the rains in the mountains. In some localities along the

western edge of the Colorado Desert, the rounded water-worn pebbles which strew the surface are beautifully polished, from the action of the sharp dry sand driven furiously over the gravelly pavement by the prevailing westerly winds; and in several of the gorges or passes through the Sierra Nevada, in the same neighbourhood, the fixed rocks of the mountains are themselves smoothed and striated by the same agency.

E. The Pacific Mountain Chain.

The third, or western belt, of the great elevated mountain zone of Western North America, is the oceanic chain which runs adjacent to the Pacific from Russian America to the peninsula of Lower California. The northern section of this chain traversing Russian and British America, is sometimes called the Pacific Alps; the middle section, from Frazer's River to the Klamath, the Cascade Range; and the southern section, lapping past part of the southern end of the latter, and extending from the Columbia River to about lat. 35°, and becoming extinct in the San Barnadino country, is called the Sierra Nevada. The central ridge of the peninsula of Lower California, a part of the same great Pacific chain, is the southward prolongation of a parallel mountain axis, which, in Middle and Northern California ranges parallel to the Sierra Nevada, west of it, and close to the Pacific coast, and is there known as the Coast Mountains. Low in its southern sections, this great mountain chain in Middle California, Oregon, and Russian America is broad, complex, and very lofty, its main central crests containing nearly the highest summits upon the continent. The Sierra Nevada, a great watershed insulating the high rainless plateau of Utah from the basin of California and from the sea, carries a very elevated crest, many parts of which reach a height of 10,000 feet; but it has few insulated peaks towering above this line. The Cascade range, on the contrary, wholly different in its geological structure, being largely volcanic, is cleft nearly to the sea level in many places, and yet bears some of the most colossal conical summits to be met with on the globe. The three great volcanic peaks, Mount Jefferson, Mount Hood, and Mount St. Helen's, tower in great masses to the height of 15,500 feet, and even above this. Mount Fair-weather, 14,782 feet high, and Mount St. Elias 17,850 feet, are both volcanoes, believed to be occasionally active; while Mount St. Helen's and Mount Regnier, though rather torpid, are known to be occasionally in eruption. About latitude 35°, the Sierra Nevada and the coast range diverge northward, to enclose between them the gold-producing valley of California.

The geological constitution of the Pacific chain differs in its different sections. The Sierra Nevada, peninsular chain and coast mountains, consist largely of the azoic or semi-crystalline strata, with belts of the gneissic and true plutonic rocks, in some parts of

their higher crests, and in less proportion, the older palæozoic rocks low upon their flanks. The Cascade chain, commencing in the huge extinct volcano Mount Shaste, and crossing the Klamath, Columbia, and Frazer rivers, dividing the northern desert from the Pacific slope, is composed entirely of comparatively recent volcanic emissions, though in some tracts it contains also belts of the older crystalline rocks. This mixed volcanic and plutonic character appears to characterise this chain throughout its long course into Russian America.

F. The Pacific or Western Slope.

Between the great Pacific chain and the western shore of the continent there extends the very long and comparatively narrow Pacific slope, everywhere declining, more or less steeply, to the sea. Slender in the Californian peninsula, it widens north of lat. 34° to admit the coast mountains and the gold-bearing valley of upper California, here presenting three subordinate belts, covering a breadth of about 100 miles, which dimension it maintains to Vancouver's Island, and beyond it. From the valley of the Sacramento, northward, its surface becomes more rugged.

The geological constitution of the Pacific slope is considerably more diversified than that of the Atlantic slope, descending eastward from the Appalachians. It consists generally of tertiary strata, mainly of miocene age: eocene, pliocene, and pleistocene, having also been recognised. These tertiaries are generally undulated and broken from violent crust movements, and in some quarters they are extensively penetrated by eruptive volcanic rocks; in other tracts the older azoic and palæozoic rocks, and still more ancient gneissic strata, forming the spurs of the coast range, intrude themselves through the tertiaries, which have probably been deposited around these ancient masses while they were above the waters.

G. The Atlantic or Eastern Slope.

This long and slender zone stretches from the Gulf of St. Lawrence to the Gulf of Mexico, and descends with a gentle lateral slope from the base of the Appalachians to the Atlantic coast. Its several subordinate belts differ both in physical features and geological structure. One sub-division, ranging from the Gulf of St. Lawrence to the Hudson, through New Brunswick and New England, has a hilly surface, and many rivers, which meet the tidal level far inland from the sea. It contains short chains of rounded hills, some of them mountains in size, and it is spotted with a multitude of clear lakes and ponds, and is throughout admirably watered. This region forms one general slope to the sea, not being fringed on its ocean border, as the corresponding zone further south is, by a true tide-water tertiary plain.

In its geological composition, this north-eastern division of the Atlantic slope consists generally of the older crystalline rocks, with

palæozoic strata resting upon them. Its entire surface is covered with northern pleistocene drift, to so great a depth in some tracts as effectually to conceal the formations underneath. The limpidness of its streams and lakes must be attributed mainly to the filtering action of this sandy and gravelly boulder drift, which contains but little dispersed clay, though beds of pure brick clay regularly stratified do occasionally occur in it.

The south-western section of the Atlantic slope, expanding south-westward from the Hudson, attains in Virginia a width of at least 200 miles, under which it continues to the southern end of the Appalachians. It includes two parallel belts, quite distinct in their geology and scenery: that nearest the mountains is a true slope, descending from a height of several hundred feet to the level of the tide; that bordering the sea is a low monotonous and very level plain. Between the slope and the plain runs a well defined line of demarcation, indicated in a sudden change in the topography, and in the abrupt transition in all the streams, from rapids to the ebb and flow of the tide. Upon this physical boundary are seated all the chief cities of the Atlantic sea-board, from Trenton, in New Jersey, to Halifax, in North Carolina. This line marks, indeed, an ancient sea coast of the earlier continent in the times anterior to the elevation of the horizontal tertiary deposits.

The upper, or Appalachian division of the Atlantic slope, traced south-westward, ascends from the level of the tide at the Hudson, to its maximum height of 1000 feet near the sources of the Roanoke, and declines again beyond the Catawba, to a more moderate level in middle Alabama. From Long Island Sound to the Potomac its surface is varied and pleasing, and its soil very fertile.

Geologically, this belt includes tracts of the gneissic series, and synclinal troughs of the most ancient palæozoic formations—all in a more or less metamorphic condition. These are overlaid by a zone of middle secondary red sandstone, of older Jurassic age. This zone embraces low rugged wooded ridges, and hills of trappean and other gneous rocks. It possesses many broad, fruitful, and salubrious valleys; is singularly well watered by clear running brooks, but is nowhere marshy, and is altogether the garden spot of the Atlantic border of the continent.

From the Potomac, south-westward, the features of this slope are more monotonous, the soil less fertile, consisting of light sands and meagre clays, produced from the subjacent talcose and chloritic schists, and other altered azoic rocks. The climate is less temperate, being liable to great heat in summer, and to heavy rains, which wash its pulverulent soil. These conditions of soil and climate impart excessive turbidness to its swiftly flowing streams, causing indirectly extensive sand bars and shoals at the mouths of its rivers and estuaries, tending to block the navigation. Immediately at the base of the blue ridge, in Virginia and North Carolina, this region contains highly fertile and picturesque tract.

The Atlantic Plain, or sea-board belt of the Atlantic slope, nowhere rises above 100 feet from the ocean level. From Long Island to North Carolina, though intersected by many tidal creeks, it is not marshy, except near the ocean, and bordering the estuaries of the Delaware and Chesapeake; but farther to the south-west, through North and South Carolina, Georgia, and Florida, its seaward half is excessively swampy and much overflowed.

Geologically, this ocean border is composed exclusively of cretaceous and tertiary deposits: the former consisting of clays and sands, including thick wide-spread beds of pulverulent green sand, greatly valued as a fertiliser for the soil; the latter, or tertiary, of sands, clays, and beds of shell marl, with few or no concreted rocky layers. The cretaceous strata outcrop along the continental side of this plain in a narrow zone, extending from the northern sea coast of New Jersey to the Chesapeake, and reappear again low upon some of the rivers of the Carolinas and Georgia; but, throughout a large part of the plain, they are deeply covered by the tertiaries. The eocene tertiary strata have a narrow long line of outcrop in a corresponding position along the western edge of the plain, from the Potomac to the Cape Fear River; they reappear again from under the middle tertiary beds, in a more central position in the plain, from the Cape Fear River to the Altamaha. Still farther to the south-west, these eocene beds fringe the southern edge of the cretaceous regions of Georgia, Alabama, and Mississippi, south of the termination of the Appalachian Mountains. This wide southern tract of eocene extends southwards into the interior of the peninsula of Florida, showing this peninsula to have originated as early at least as the morning period of the tertiary day.

The miocene tertiaries cover nearly the whole of the tide-water plain, except the narrow eocene strip on the west, and a pliocene area on the sea coast of Virginia and North Carolina, from southern New Jersey, through Delaware, Maryland, Virginia, and North Carolina.

The pliocene deposits skirt, at intervals, the entire Atlantic sea coast from the Chesapeake to Florida, and also the whole coast of the Gulf of Mexico, from Florida to Texas; forming, for the most part, a fringe to the eocene beds, the miocene not having been deposited in these regions.

Geological Features of the United States.

Turning, next, to a more special description of the geological features of the United States, the speaker sketched briefly the great natural areas occupied by its separate formations.

1st. To the north of the east and west Lawrentine watershed, which itself consists generally of the ancient crystalline rocks, including the older metamorphic or gneissic, later metamorphic or azoic, or non-fossiliferous palæozoic strata, and many plutonic outbursts;—there extends to a high latitude, and filling a large part of

the natural hydrographic basin of Hudson Bay, an area or basin of fossiliferous palæozoic strata of the upper Silurian, Devonian, and possibly Carboniferous periods. This may be called the Hudson Bay, or Arctic Palæozoic Basin.

To the south of the before-mentioned Lawrentine igneous watershed, and westward from the Atlantic slope to the Rocky Mountains, and even to the great Pacific chain, and probably north-westward to the basin of Mackenzie River, there spreads another still larger palæozoic basin. The south-eastern and best-developed part of this area, from the Appalachians to the plains of Kanzas and Nebraska, entitled the Appalachian Basin, includes formations of all the palæozoic periods known to geologists, from the dawn of life upon the globe, to the close of the age of the coal.

From this brief statement of the two basins, and an inspection of the geological map, it appears that the Appalachian Basin, or that south of the Lawrentian Lakes, was depressed or under water in the earlier palæozoic periods, while the region north of the Lawrentine watershed was above the level of the sea. But at the close of the Cambrian or older Silurian ages, that great disturbance of the crust, which let the ocean in upon the area of the present basin of Hudson Bay, for the production of the Silurian and later strata, lifted out a part, and shallowed other portions of the sea-bed of the other, or Appalachian Basin, to the south. This is manifested in a break in the sequence of the strata, wherever the older Silurian or Cambrian, and the later palæozoic rocks are there recognisable together.

The first stage, then, in the physical geography of the primeval North America, was the existence of a small northern continent, the southern coast of which was nearly coincident with the northern skirt of the present valley of the St. Lawrence and its lakes. This continent, or nucleus of one, sent forward to the south a long peninsular tract, the vestiges of which we may discern in the hypozoic and azoic belt of the Atlantic slope, stretching from New Brunswick to Georgia. Very possibly other lands lay to the eastward of this region of the Atlantic slope at that early date, and were depressed during some of the earlier oscillations of the crust in this quarter of the hemisphere; the seeming eastern origin of many of the Appalachian palæozoic strata, to the coal rocks inclusive, is eminently suggestive of the existence in the palæozoic times, of some such large tract of land, an antient Atlantis, now under the bed of the western part of the Atlantic.

The next general movement of the crust, in the region now constituting the eastern half of North America, was at the end of the coal period, manifestly an epoch of very extensive uplift of the continent. This shift of level, and total drainage of the eastern half of the Appalachian sea-bed, caused a large accession to the continent, the new shore of which, if assumed to be coincident with the line which now separates the palæozoic Appalachian formations, and yet older ones of the Atlantic slope, from the later horizontal cretaceous

and tertiary deposits that fringe them, was that well-marked physical limit already partially traced as the inner edge of the low tertiary and cretaceous plain. Probably, however, this newly-produced part of the continent, particularly on its western side, was somewhat more extended at the date of elevation than the present margin of the cretaceous formation indicates; for it is upon this supposition, coupled with a belief that the newly uplifted formations remained out of water, under somewhat wider boundaries than they now exhibit, that we can best explain the non-existence of any Permian and Triassic formations between the Carboniferous, the latest of the American palæozoics, and the Cretaceous, the next more recent sediments deposited against them. During all the long geological ages which intervened between the lifting out the palæozoic region of the United States east of the Missouri, and the deposition of the Cretaceous strata, no sedimentary formations of the Mesozoic periods, such as those which fill large tracts in other quarters of the globe, were permanently upraised into dry land, saving only a few narrow strips—products of estuary sedimentation—stretching at intervals along the Atlantic slope from Prince Edward's Island to Carolina. Elsewhere, certainly as far westward as the Rocky Mountains, either nothing was deposited during the permian, triassic, and jurassic ages, or, what is far more probable, the formations then produced were formed outside of the present palæozoic limits, and have been covered up from sight by the wider sediments of the cretaceous sea, which, lapping over them, have shut in all the earliest border tracts of the palæozoic lands, formed at the end of the coal period.

The nature of the crust movements which elevated the palæozoic strata was in the region of maximum disturbance—that of the Atlantic slope and Appalachian chain,—a stupendous undulation or wave-like pulsation, the strata being elevated into permanent anticlinal and synclinal flexures, remarkable for their wave-like parallelism, and for their steady declining gradation of curvature, when they are compared in any east and west section across the corrugated zone. To the westward of the Appalachian chain, where this structure is so conspicuous, the crust waves flatten out, recede from each other, and vanish into general horizontality; and this nearly level condition extends thence throughout all the older rocks to the plains of Texas and Nebraska, where the cretaceous beds overlap them. The orographic features of the Appalachian chain, even to the minutest slopes and terraces upon the flanks of the ridges, are all beautifully impressive of the carving action of deep retreating waters.

The cretaceous deposits of the United States all imply a marine origin, no fresh water remains having hitherto been discovered among them; and the geological map now exhibited pictures approximately the wide extent of the cretaceous, or later mesozoic sea, as it washed the boundaries of the palæozoic continent. The shore of that sea was, as before hinted, the inner edge of the Atlantic

plain on the east; on the south it was the southern termination of the Appalachian chain and the other great north-east and south-west palæozoic tract, west of the present Mississippi; and on the west, for a long distance northward, it coincided generally with the present valley of the Upper Missouri. This cretaceous sea lapped round the southern end of the Rocky Mountains, and spread as far to the west as the Cordilleras of New Mexico and the Wahsatch chain of Utah. We do not at present know that it extended any further.

The close of the cretaceous or chalk period was marked by the rise and desiccation of nearly the whole now continental area of this great northern Mediterranean. The movement along the Atlantic border of the continent was comparatively slight, for it brought above the sea level only very limited and narrow tracts of the shoal water cretaceous sediments. South of the Appalachian region it was somewhat more extensive, elevating a wider zone between the previous dry land and the newly formed tertiary shore; but to the west of the region of older rocks it was a broad continental rising, draining dry nearly the whole bed of the then existing ocean to the limit indicated beyond the Rocky Mountains. The coast line established by this lift of the crust, set new and much more restricted bounds westward and northward to the Atlantic ocean, and established the outlines of the present Gulf of Mexico, which thus dates back as far as the commencement of the eocene tertiary age. It does not seem probable that Florida was then any part of the dry land, the true southern peninsula of the continent being rather the newly formed cretaceous plain at the end of the Appalachians.

From that date, the movements in the level of the continent have been manifestly less and less. Its great outlines, established at the close of the chalk period, have remained as its contour to the present day; and each successive gain of territory, during the several tertiary revolutions—that which ended the eocene, that which closed the miocene, and that which cut off the pliocene, and even the pleistocene deposits, was but an enlargement of the primitive mesozoic pattern, a mere addition of a lighter and lighter fringe to the broad mantle of land, which earlier convulsions had constructed.

American Coal Fields.

The speaker selected from the many topics presented by this sketch of the geology of North America, that of the coal-fields of the United States and British Provinces, as presenting a theme of general interest, describing first briefly the carboniferous formations, especially their coal measures.

This formation consists, in the United States and North-eastern British provinces, of argillaceous and siliceous sandstones, conglomerates, clay shales, fire clays, and coal slates; argillaceous limestones, chiefly of marine origin, and seams of coal. A coarse siliceous conglomerate or *millstone grit*, generally destitute of coal, underlies the

productive coal measures throughout nearly all the different basins, proving the universality of the action which attended the commencement of that state of the physical geography that witnessed the production of the coal seams and the sediments which enclose them.

The eastern half of the continent exhibits five great coal fields, extending from Newfoundland to Arkansas. 1. The first, or most eastern, is that of the British provinces, Newfoundland, Nova Scotia, Cape Breton, Prince Edward's Island, and New Brunswick. This seems to have been originally one wide coal-field, subsequently broken up into patches by upheaval and denudation, and by the submergence which formed the Gulf of St. Lawrence: the area of the coal measures of the provinces is probably about 9000 square miles, though only one-tenth of this surface appears to be underlaid by productive coal seams. 2. The second, or great Appalachian coal-field, extends from North-eastern Pennsylvania to near Tuscaloosa, in the interior of Alabama. It is about 875 miles long, and 180 broad, where widest in Pennsylvania and Ohio, and by a careful estimate contains about 70,000 square miles. The narrow basins of anthracite in eastern Pennsylvania, containing less than 300 square miles of coal, are outlying troughs from this great coal-field. 3. A third, smaller coal-field, occupies the centre of the state of Michigan, equidistant from Lake Huron and Michigan; it covers an area of about 15,000 square miles, but it is very poor in coal. 4. A fourth great coal-field is that situated between the Ohio and Mississippi anticlinals, in the States of Kentucky, Indiana, and Illinois. It has the form of a wide elliptical basin. It is about 370 miles long, and 200 miles wide, and contains by estimation 50,000 square miles of coal measures. 5. The fifth, and most western, is the large and very long coal-field filling the centre of the great basin of carboniferous rocks which spreads from the Mississippi and Ozark anticlinals, westward to the limit of the palæozoic region, where the cretaceous strata begin. The coal-field itself has its northern limit on the Iowa River, and its southern near the Red River, on the western border of Arkansas. It is in length 650 miles, and in greatest breadth 200 miles. The total area of this great irregular basin is probably not less than 57,000 square miles. Three or more small detached tracts of coal strata, encompassed by the cretaceous deposits, stretch at intervals south-westward from the southern limit of the longer field through Texas. They are probably extensions of the great field laid bare by denudation. Other localities of coal-bearing strata occur in the high table lands on both sides of the Rocky Mountains, and also in the Wahsatch chain of Utah, but it is doubtful whether any of them belong to the true carboniferous series. The aggregate space underlaid by these vast fields of coal amounts to at least 200,000 square miles, or to more than twenty times the area which includes all the known coal deposits of Europe, or indeed, of the whole eastern continent.

These coal-fields, especially the four lying west of the Atlantic slope, exhibit several interesting facts of gradation, which render it highly probable that they were, at one time, all of them connected, the vacant intervals now separating them having been denuded of their coal measures by the wide-sweeping erosive action of the waters of the Appalachian Sea, set in motion at the uprising of this part of the continent. The first fact of such gradation relates to the thickness of the formation, and that of the individual coal seams in the respective coal-fields, the comparison indicating a marked reduction in this respect from east to west. Thus the productive coal measures of Nova Scotia have a thickness of nearly 3000 feet, those of the anthracite basins of Pennsylvania a depth about as great, while those in the central parts of the great Appalachian basin show a thickness not exceeding 2500 feet. Again, in the Illinois basin the probable thickness is reduced to 1500 feet, while in the farthest, or Ohio and Missouri basin, it cannot exceed 1000 feet. Very similar is the reduction in the number of the coal seams. Those at the Joggins, in Nova Scotia, are about 50; though only five of them are of workable dimensions, being equivalent to about 20 feet of coal. The deepest anthracite basin of Pennsylvania, that of the Schuylkill, contains also about 50 coal seams, but 25 of these have a thickness each of more than three feet, and are available for mining. Further west, the great Appalachian coal-field contains about 20 beds in all, 10 of which are thick enough to be mined. Still farther onward, the broad basin of Indiana and Illinois shows apparently not more than 10 or 12 beds, and it is believed that only 7 of these are thick enough and pure enough for mining. Northward, in the Michigan coal-field, denudation has left only the two or three lower beds. Still further westward the coal-field of Iowa and Missouri contains, it is believed, but 3 or 4 beds of profitable size, and the total number, thick and thin, does not exceed 6 or 7. A similar gradation is noticeable in the general size of the individual coal seams, by far the thickest being in the anthracite basins of eastern Pennsylvania.

Parallel with this progressive reduction in the amount of land-derived material in the upper coal formation, is a diminution in the coarseness of the mechanical ingredients of the strata, the eastern coal measures having more conglomerates and coarse sandstones, the western, more fine-grained argillaceous sandstones and clay beds; and as a further indication that the first land lay to the east, and the ocean to the west, of the wide coal-producing plains or meadows, there is with this westward reduction of the mechanically derived sediments from the land, a steady augmentation of marine limestones, and other true aqueous deposits, precipitates of a shallow carboniferous sea. In some of the more western coal-fields the alternation of the terrestrial coal seams with super-imposed limestones, containing marine fossils, amounts even to an occasional actual contact of the two kinds of strata.

Apart from these general phenomena of gradation which belong to the conditions under which the strata originated, there exist other facts of transition, which also imply a declension westward, of quite another class of forces than the productive ones. In the Appalachian chain, all the eastern coal basins, and the eastern margin of the great Appalachian field, give evidence of a crust metamorphism, affecting all the palæozoic rocks, the degree of alteration dependant on the nature of the stratum, and its situation, eastward or westward, in this great undulated zone. The coal, of all rocks, the most sensitive to metamorphism from heat, presents invariably among the eastern flexures of the Appalachians, where the crust action has been greatest, the condition of a hard and flinty anthracite with a jaspery or large conchoidal fracture; further westward in the same set of basins, the anthracite has a more cuboidal fracture, is softer, and is even slightly gaseous. Entering the eastern border of the great Appalachian bituminous coal-field, we everywhere find the coal possessed of only half its full share of volatile or gaseous matter; and it is not until we reach the middle and western side of this wide basin, that the coal is found fully bituminous,—in other words, not until we pass beyond the last of the perceptible undulations of the crust. Throughout all the flat fields of the Western States, the coal invariably retains its original full amount of bituminous or gaseous ingredients.

Comparing the areas of the coal-fields of other countries with those of North America now indicated, Great Britain may be estimated to contain about 5400 square miles, France 1000, and Belgium 510 square miles. Rhenish Prussia—Saarbrook field—has 960 square miles, Westphalia 380, the Bohemian field, about 400; that of Saxony, only 30; that of the Asturias, in Spain, probably 200; and that of Russia, scarcely 100 square miles. And as these are the principal known coal-fields in Europe, the whole is thus seen to possess less than 9000 square miles of productive coal measures. Comparing the coal areas with the total areas of the respective countries, the United States has one square mile of coal-field to each 15 square miles of its 3,000,000 miles of territory; Great Britain has one square mile to each 22½ of surface; Belgium a like proportion; while France possesses only one square mile of coal-field to every 200 miles of country. Assuming the total area of the productive coal measures of the world at 220,000 square miles, and accepting 20 feet as the average thickness of the available coal, the entire quantity, if estimated as one lump, is equivalent to a cube of very nearly 10 miles dimensions, or equivalent to a cake or plateau of coal 100 miles square in its base, and 440 ft. high.

The present annual product of the chief coal producing countries is as follows:—Great Britain extracted from her coal mines last year—1855—the enormous quantity of 65,000,000 tons; the United States, between 8 and 9,000,000; Belgium, about 5,000,000; and France, 4,500,000.

It is interesting to compare the dynamic force of coal applied as fuel to the generation of steam in the steam-engine, with the dynamic effect of a man. The human labourer, exerting his strength upon a treadmill, can raise his own weight, say 150 lbs., through a height of 10,000 ft. per day, equivalent to 1 lb. raised 1,500,000 ft. The mechanical virtue of fuel is best estimated by ascertaining the number of pounds which a given quantity, say one bushel, will raise to a given height, say one foot, against gravity. In the steam-engine this is called the *duty of the fuel*. Now, the present maximum duty of one bushel of good coal in the improved Cornish steam-engines, is equivalent to 100,000,000 lbs. lifted through one foot; but one bushel has been made to raise 125,000,000 lbs. one foot high, or one pound 125,000,000 of feet; but as there are 84 lbs. in one bushel, this divisor gives 1 pound as equal to 1,500,000 ft.; just the result of a man's toil for one day upon a treadmill. Thus a pound of coal is really worth a day's wages. If we estimate a lifetime of hard work at 20 years, giving to each year 300 working days, we have for a man's total dynamic effort 6000 days. In coal this is represented by the amazingly small amount of *three tons*. Another proof of the extraordinary power derivable through the combustion of fuel, is presented in the following calculation; one cubic inch of water is convertible into steam, of one atmospheric pressure, by 15$\frac{1}{3}$ grains of coal, and this expansion of the water into steam is capable of raising a weight of one ton the height of a foot. The one cubic inch of water becomes very nearly one cubic foot of steam, or 1728 cubic inches. When a vacuum is produced by the condensation of this steam, a piston of one square inch surface, that may have been lifted 1728 inches, or 144 feet, will fall with a velocity of a heavy body rushing by gravity through one half of the height of the homogeneous atmosphere, or through 13,500 feet. This gives a terminal velocity of 1300 feet per second, greater than that of the transmission of sound. From this we can form some estimate of the strength of the tempest which alternately blows the piston in its cylinder, when elastic steam of high pressure is employed. Applying the calculations of the dynamic efficiency of coal, for estimating the mechanical strength latent in the coal-fields of the earth, or in the large coal product annually furnished by the mines of Great Britain, we get some interesting results. Each acre of a coal seam, four feet in thickness, and yielding one yard nett of pure fuel, is equivalent to about 5000 tons; and possesses, therefore, a reserve of mechanical strength in its fuel equal to the life-labour of more than 1600 men. Each square mile of one such single coal bed contains 3,000,000 of tons of fuel: equivalent to 1,000,000 of men labouring through twenty years of their ripe strength. Assuming, for calculation, that 10,000,000 of tons, out of the present annual product of the British coal mines, namely 65,000,000, are applied to the production of mechanical power, then England annually summons to her aid an army of

3,300,000 fresh men, pledged to exert their fullest strength through 20 years. Her actual annual expenditure of power, then, is represented by 66,000,000 of able-bodied labourers. The latent strength resident in the whole coal product of the kingdom may, by the same process, be calculated at more than 400,000,000 of strong men, or more than double the number of the adult males now upon the globe.

Climates.—Adverting to the causes of the characteristic features of the North American climates, those of all the eastern and northern divisions of the continent were shown to depend primarily upon the peculiar distribution of the land and water, and the general circulation of the winds and oceanic currents in the North Atlantic and the Polar Basins, resulting from general phenomena of rotation of the fluids, and from the configuration of those seas. The chief surface currents of both these basins belong all to one great circulating stream, which, crossing the Atlantic from Africa to the Gulf of Mexico, under the northern tropic, and following for a vast distance the highly-heated shores of South and Central America, enters the North Atlantic at Florida, under the name of the Gulf Stream, carrying a temperature 5° to 6° higher than the mean heat of the equator, and imparting to the southern coast of the United States the ocean temperature of the tropics. Pursuing its career to the north-east, this current transports its own mild climate to the whole north-western side of Europe, and even subdues the rigours of the European Polar sea; but refrigerated, as it sweeps round in its circumpolar course, the shores of Siberia and Western Arctic America; and, loaded with the annual ice of all that extended zone, it streams through the great Archipelago of North-eastern Arctic America, clogs its deep channels with its floating packs of ice, and chills to the zero temperature of the whole hemisphere this coldest of all the summer climates of the globe. Returning into the Atlantic around Greenland, and by its main passage through Baffin Bay, this now arctic ice-chilled and ice-transporting current, hugs the whole north-eastern coast of the continent inside of the Gulf Stream. It thus weaves a track somewhat resembling the figure 8. The Gulf Stream on the south-east, and the Arctic current on the north, conjointly with the tropical and the polar winds with which they are connected, produce such a contrast in the temperature of the southern and northern latitudes of eastern America, that all the zones of the climates of the sphere are there compressed within not more than 30° of a great circle, crossing the continent from the Gulf Stream to lat. 70° north of Hudson Bay.

The climatology of the western half of the continent was next discussed. There the controlling agent in the latitudes north of the north-east Trade Wind of the tropic, is the south-west and west wind from the Pacific Ocean, and in the Southern Atlantic States from the Gulf of Mexico. This Pacific Ocean wind, moderately charged with moisture in the lower latitudes, and excessively humid in the

more northern ones from traversing a wider tract of sea, confers a temperate, moist, and oceanic climate upon the Pacific slope; but deprived largely of its moisture by ascending the high mountain barrier of the Pacific chain, which robs it of nearly the whole of its wetness, it exerts in the more southern latitudes just the opposite effect upon the plains and table lands to the leeward or eastward of that barrier. The high evaporative power of the winds thus parched accounts for the excessive aridity of the Colorado and Utah deserts, and the whole desert belt of the interior, which in the lower latitudes stretches to Texas. It likewise explains the prevalence of numerous salt lakes, destitute of outlets, and the occurrence of the wide tracts covered with salt, or with a saline soil, within this area.

Gold.—To the same general cause, the Pacific wind, we are to attribute the abundant gold alluvia of the western slope and base of the Sierra Nevada. The copious precipitation of rain, amounting to nearly the whole humidity of the Pacific wind, against the gold-containing western flank of the Californian chain, greater probably in the pleistocene than in the modern epochs, has produced an enormous erosion of the gold-bearing and cleavage fissured rocks; and has strewn and sorted their fragments and particles in the ravines of the mountain, and in the plains at its base. The speaker concluded with an announcement of the general fact that, whereas the salt-fields of the earth are found upon the continental or interior dry sides of its oceanic chains, its gold-fields are restricted to their wet or oceanic slopes.

[H. D. R.]

WEEKLY EVENING MEETING,

Friday, February 15.

SIR HENRY HOLLAND, Bart. M.D. F.R.S. Vice-President, in the Chair.

THOMAS HENRY HUXLEY, Esq. F.R.S.
FULLERIAN PROFESSOR OF PHYSIOLOGY, R.I.

On Natural History, as Knowledge, Discipline, and Power.

THE value of any pursuit depends upon the extent to which it fulfils one or all of three conditions. Either it enlarges our experience; or it increases our strength; or it diminishes the obstacles in the way of our acquiring experience and strength. Whatever neither teaches, nor strengthens, nor helps us, is either useless or mischievous.

The scientific calling, like all others, must be submitted to these tests, if we desire fairly to estimate its dignity and worth; and as the object of the present discourse is to set forth such an estimate of the science of Natural History, it will be necessary to consider—Firstly, its scope and range as mere *knowledge;* Secondly, the amount to which the process of acquiring Natural History knowledge strengthens and developes the powers of the gainer,—its position, that is, as *discipline;* Thirdly, the extent to which it enables him, so to speak, to turn one part of the universe against another, in order to attain his own ends; and this is what is commonly called the *power* of science.

There can be little doubt as to which is the highest and noblest of these standards of value. Science, as power, indeed, showers daily blessings upon our practical life; and science, as knowledge, opens up continually new sources of intellectual delight. But neither knowing nor enjoying are the highest ends of life. Strength—capacity of action and of endurance—is the highest thing to be desired; and this is to be obtained only by careful discipline of all the faculties, by that training which the pursuit of science is, above all things, most competent to give.

First, let us regard Natural History as mere Knowledge.

The common conception of the aims of a naturalist of the present day does him great injustice, although it might perhaps fairly apply to one of a century and a half ago; when natural history, which began in the instinctive observation of the habits, and the study of the forms, of living beings, had hardly passed beyond the stage of more or less accurate anecdotes, and larger or smaller collections of curiosities.

The difference between the ancient naturalist and his modern successor, is similar to that between the Chaldæan watcher of the stars and the modern astronomer; but the scientific progress of the race is epitomized in that of the individual, and may be best exemplified, perhaps, by tracing out the lines of inquiry into which any person of intelligence, who should faithfully attempt to solve the various problems presented by any living being, however simple and however humble, would necessarily be led. By the investigation of habits, the inquirer is insensibly led into Physiology, Psychology, Geographical and Geological distribution; by the investigation of the relations of forms, he is no less necessarily impelled into systematic Zoology and Botany, into Anatomy, Development, and Morphology, or Philosophical Anatomy. Now each of these great sciences is, if followed out into all its details, the sufficient occupation of a lifetime; but in their aggregate only, are they the equivalent of the science of natural history: and the title of naturalist, in the modern sense, is deserved only by one who has mastered the principles of all.

So much for the range of natural history. If we consider, not merely the number, but the nature of the problems which it presents,

we shall find that they open up fields of thought unsurpassable in interest and grandeur. For instance, morphology demonstrates that the innumerable varieties of the forms of living beings are modelled upon a very small number of common plans or types. ("*Haupt-Typen*," of Von Bär, whose idea and term are merely paraphrased by "archetype," common plan, &c.) In the animal world we find only five of these common plans, that of the *Protozoa*, of the *Cœlenterata*, of the *Mollusca*, of the *Annulosa*, and of the *Vertebrata*. Not only are all animals existing in the present creation organized according to one of these five plans; but palæontology tends to show that in the myriads of past ages of which the earth's crust contains the records, no other plan of animal form made its appearance on our planet. A marvellous fact, and one which seems to present no small obstacle in the way of the notion of the possibly fortuitous development of animal life.

Not merely does the study of morphology lead us into the depths of past time, but it obliges us to gaze into that greater abyss which lies between the human mind and that mind of which the universe is but a thought and an expression. For man, looking from the heights of science into the surrounding universe, is as a traveller who has ascended the Brocken and sees, in the clouds, a vast image, dim and awful, and yet in its essential lineaments resembling himself. In the words of the only poet of our day who has fused true science into song, the philosopher, looking into Nature,

> "Sees his shadow glory-crowned,
> He sees himself in all he sees."
>
> *Tennyson's "In Memoriam."*

The mathematician discovers in the universe a "Divine Geometry;" the physicist and the chemist everywhere find that the operations of nature may be expressed in terms of the human intellect; and, in like manner, among living beings, the naturalist discovers that their "vital" processes are not performed by the gift of powers and faculties entirely peculiar and irrespective of those which are met with in the physical world; but that they are built up and their parts adapted together, in a manner which forcibly reminds us of the mode in which a human artificer builds up a complex piece of mechanism, by skilfully combining the simple powers and forces of the matter around him. The numberless facts which illustrate this truth are familiar to all, through the works of Paley and the natural theologians, whose arguments may be summed up thus—that the structure of living beings is, in the main, such as would result from the benevolent operation, under the conditions of the physical world, of an intelligence similar in kind, however superior in degree, to our own. Granting the validity of the premises, that from the similarity of effects we may argue to a similarity of cause, does natural history allow our conclusions to rest here? Is this utilitarian adaptation to a benevolent purpose, the chief or even the leading feature of that great shadow, or, we should more rightly say, of that vast arche-

type of the human mind, which everywhere looms upon us through nature? The reply of natural history is clearly in the negative. She tells us that utilitarian adaptation to purpose is not the greatest principle worked out in nature, and that its value, even as an instrument of research, has been enormously overrated.

How is it then, that not only in popular works, but in the writings of men of deservedly high authority, we find the opposite dogma—that the principle of adaptation of means to ends is the great instrument of research in natural history—enunciated as an axiom? If we trace out the doctrine to its fountain head, we shall find that it was primarily put forth by Cuvier—the prince of modern naturalists. Is it to be supposed then that Cuvier did not himself understand the methods by which he arrived at his great results? that his master-mind misconceived its own processes? This conclusion appears to be not a little presumptuous; but if the following arguments be justly reasoned out, it is correct.

In the famous "Discours sur les Révolutions de la Surface du Globe," after speaking of the difficulties in the way of the restoration of vertebrate fossils, Cuvier goes on to say—

"Happily, comparative anatomy possesses a principle whose just development is sufficient to dissipate all difficulties; it is that of the correlation of forms in organized beings, by means of which every kind of organized being might, strictly speaking, be recognised by a fragment of any of its parts.

"Every organized being constitutes a whole, a single and complete system, whose parts mutually correspond, and concur, by their reciprocal reaction, to the same definitive end. None of these parts can be changed without affecting the others; and consequently, each taken separately indicates and gives all the rest."

After this Cuvier gives his well-known examples of the correlation of the parts of a carnivore, too long for extract; and of which therefore his summation merely will be given:—

"In a word, the form of the tooth involves that of the condyle; that of the shoulder blade; that of the claws: just as the equation of a curve involves all its properties. And just as by taking each property separately and making it the base of a separate equation, we should obtain both the ordinary equation, and all other properties whatsoever which it possesses; so, in the same way, the claw, the scapula, the condyle, the femur, and all the other bones taken separately will give the tooth, or one another; and by commencing with any one, he who had a rational conception of the laws of the organic economy, could reconstruct the whole animal."

Thus far Cuvier: and thus far and no further, it seems that the compilers, and copyers, and popularizers, and *id genus omne*, proceed in the study of him. And so it is handed down from book to book, that all Cuvier's restorations of extinct animals were effected by means of the principle of the physiological correlation of organs.

Now let us examine this principle; taking in the first place, one

of Cuvier's own arguments and analyzing it; and in the second place, bringing other considerations to bear.

Cuvier says—"It is readily intelligible that ungulate animals must all be herbivorous, since they possess no means of seizing a prey (1). We see very easily also, that the only use of their fore feet being to support their bodies, they have no need of so strongly formed a shoulder; whence follows the absence of clavicles (2) and acromion, and the narrowness of the scapula. No longer having any need to turn their fore-arm, the radius will be united with the ulna, or least articulated by a ginglymus and not arthrodially with the humerus (3). Their herbivorous diet will require teeth, with flat crowns, to bruise up the grain and herbage; these crowns must needs be unequal, and to this end enamel must alternate with bony matter (4); such a kind of crown requiring horizontal movements for trituration, the condyle of the jaw must not form so close a hinge as in the carnivora; it must be flattened; and this entails a correspondingly flattened temporal facet. The temporal fossa which will have to receive only a small temporal muscle will be shallow and narrow (5)."

The various propositions are here marked with numbers, to avoid repetition; and it is easy to show that not one is really based on a necessary physiological law:—

> (1.) Why should not ungulate animals be carrion feeders? or even, if living animals were their prey, surely a horse could run down and destroy other animals with at least as much ease as a wolf.
>
> (2, 3.) But what purpose, save support, is subserved by the forelegs of the dog and wolf? how large are their clavicles? how much power have they of rotating the fore-arm?
>
> (4, 5.) The sloth is purely herbivorous, but its teeth present no trace of any such alternation of substance.

Again, what difference exists in structure of tooth, in the shape of the condyle of the jaw, and in that of the temporal fossa, between the herbivorous and carnivorous bears? If bears were only known to exist in the fossil state, would any anatomist venture to conclude from the skull and teeth alone, that the white bear is naturally carnivorous, while the brown bear is naturally frugivorous? Assuredly not; and thus, in the case of Cuvier's own selection, we see that his arguments are absolutely devoid of conclusive force. Let us select another then; on the table is a piece of carboniferous shale, bearing the impression of an animal long since extinct. It is a mere impression of the external form, but this is amply sufficient to enable us to be morally certain that if we had a living specimen, we should find its jaws, if it had any, moving sideways—that its hard skeleton formed a sheath outside its muscles—that its nervous system was turned downwards when it walked—that the heart was placed on the opposite side of the body—that if it possessed special respiratory organs, they were gills, &c. &c.

In fact we have in the outward form abundant material for the

restoration of the internal organs. But how do we conclude, from the peculiar many-ringed body, with jointed limbs, of this ancient marine animal, that it had all these other peculiarities; in short, that it was a crustacean? For any physiological necessity to the contrary, the creature might have had its mouth, nervous system, and internal organs arranged like those of a fish. We know that it was a crustacean and not a fish, simply because the observation of a vast number of instances assures us that an external structure such as this creature possesses, is invariably accompanied by the internal peculiarities enumerated. Our method then is not the method of adaptation, of necessary physiological correlations; for of such necessities, in the case in question, we know nothing: but it is the method of agreement; that method by which, having observed facts invariably occur together, we conclude they invariably have done so, and invariably will do so; a method used as much in the common affairs of life as in philosophy.

Multitudes of like instances could be adduced from the animal world; and if we turn to the botanist, and inquire how he restores fossil plants from their fragments, he will say at once that he knows nothing of physiological necessities and correlations. Give him a fragment of wood, and he will unhesitatingly tell you what kind of a plant it belonged to, but it will be fruitless to ask him what physiological necessity combines, *e.g.* peculiarly dotted vessels, with fruit in the shape of a cone and naked ovules, for he knows of none. Nevertheless, his restorations stand on the same logical basis as those of the zoologist.

Therefore, whatever Cuvier himself may say, or others may repeat, it seems quite clear that the principle of his restorations was *not* that of the physiological correlation or coadaptation of organs. And if it were necessary to appeal to any authority, save facts and reason, our first witness should be Cuvier himself, who, in a very remarkable passage, two or three pages further on, (Discours, pp. 184-185,*) implicitly surrenders his own principle.

Thus then natural history plainly teaches us that the utilitarian principle, valuable enough in physiology, helps us no further, and is utterly insufficient as an instrument of morphological research.

But does she then tell us that in this, her grander sphere, the human mind discovers no reflex, and that among those forms of being which most approach himself alone, man can discover no indication of that vast harmony with his own nature which seemed so obvious elsewhere? Surely not. On the contrary, it may be regarded as one of the noblest characteristics of natural history knowledge, that its highest flights point, not to a discrepancy between the infinite and the finite mind, but to a higher and closer union than can be imagined by those whose studies are confined to the physical world. For where the principle of adaptation, of mere

* *Ossemens* Fossiles, 4me édition, T. 1.

mechanical utilitarian contrivance fails us, it is replaced by another which appeals to the æsthetic sense as much as the mere intellect.

Regard a case of birds, or of butterflies, or examine the shell of an echinus, or a group of foraminifera, sifted out of the first handful of sea sand. Is it to be supposed for a moment that the beauty of outline and colour of the first, the geometrical regularity of the second, or the extreme variety and elegance of the third, are any *good* to the animals? that they perform any of the actions of their lives more easily and better for being bright and graceful rather than if they were dull and plain? So, to go deeper, is it conceivable that the harmonious variation of a common plan which we find everywhere in nature serves any utilitarian purpose? that the innumerable varieties of antelopes, of frogs, of clupeoid fishes, of beetles, and bivalve mollusks, of polyzoa, of actinozoa, and hydrozoa, are adaptations to as many different kinds of life, and consequently varying physiological necessities? Such a supposition with regard to the three last, at any rate, would be absurd; the polyzoa, for instance, presenting a remarkable uniformity in mode of life and internal organization, while nothing can be more striking than the wonderful variety of their external shape and of the sculpture of their cells. If we turn to the vegetable world, we find it one vast illustration of the same truth. Who has ever dreamed of finding an utilitarian purpose in the forms and colours of flowers, in the sculpture of pollen-grains, in the varied figures of the frond of ferns? What "purpose" is served by the strange numerical relations of the parts of plants, the threes and fives of monocotyledons and dicotyledons?

Thus in travelling from one end to the other of the scale of life, we are taught one lesson, that living nature is not a mechanism but a poem; not a mere rough engine-house for the due keeping of pleasure and pain machines, but a palace whose foundations, indeed, are laid on the strictest and safest mechanical principles, but whose superstructure is a manifestation of the highest and noblest art.

Such is the plain teaching of Nature. But if we have a right to conclude from the marks of benevolent design to an infinite Intellect and Benevolence, in some sort similar to our own, then from the existence of a beauty (nay, even of a humour,) and of a predominant harmonious variety in unity in nature, which, if the work of man, would be regarded as the highest art, we are similarly bound to conclude that the æsthetic faculties of the human soul have also been foreshadowed in the Infinite Mind.

Such is a brief indication of the regions of thought into which natural history leads us, and we may surely conclude that as *Knowledge* it stands second in scope and breadth to no science.

As *Discipline*, impartial consideration will show that it takes no lower rank; whether we regard it as a gymnastic for the intellectual, the moral, or the æsthetic faculty.

For the successful carrying on of the business of life, no less than for the pursuit of science, it is essential that the mind should easily and accurately perform the four great intellectual processes of observation, experiment, induction, and deduction. No training can be so well adapted to develope the first of these faculties as that of the naturalist, the very foundation of whose studies lies in exact observation of characters and nice discrimination of resemblances and differences. In fact, the skilled naturalist is the only man who combines the moral and intellectual advantages of civilization with that acuteness and minute accuracy of perception which distinguish the savage hunter; and if man's senses are to keep pace with his intellect as the world grows older, natural history observation must be made a branch of ordinary education.

Again, what science can present more perfect examples of the application of the methods of experiment than physiology? All that we know of the physiology of the nervous system rests on experiment; and if we turn to other functions, the investigations of Bernard might be cited as striking specimens of experimental research.

To say that natural history as a science is equivalent to the assertion that it exercises the inductive and deductive faculties; but it is often forgotten that the so-called "natural classification" of living beings is, in reality, not mere classification, but the result of a great series of inductive investigations. In a "natural classification" the definitions of the classes are, in fact, the laws of living form, obtained, like all other laws, by a process of induction from observed facts.

For examples of the exercise of deduction, of the arguing from the laws of living form obtained by induction, to their legitimate consequences, the whole science of palæontology may be cited. As has been already shown, the whole process of palæontological restoration depends—First, on the validity of a law of the invariable coincidence of certain organic peculiarities established by induction; Secondly, on the accuracy of the logical process of deduction from this law. Professor Owen's determination of the nature of the famous Stonesfield mammal is a striking illustration of this. A small jaw of a peculiar shape, was found, containing a great number of teeth some of which were imbedded by double fangs in the jaw.

Now these laws have been inductively established—

(*a*) That only mammals have teeth imbedded in a double socket.

(*b*) That only marsupials have teeth in so great a number, imbedded in so peculiarly formed a jaw.

By deduction from these laws to the case in question, the legitimate conclusion was arrived at, that the jaw belonged to a marsupial mammal.

The naturalist then, who faithfully follows his calling leaves no side of his intellect untrained; but, after all, intellect, however *gigantic, confers* but half the qualifications required by one who

desires to follow science with success, and he who gains only knowledge from her, gains but little. The moral faculties of courage, patience, and self-denial, are of as much value in science as in life; the origin of an erroneous doctrine lies as often in the heart as in the head; and the basis of the character of a great philosopher will commonly be found, on close analysis, to be earnest truthfulness—and no imaginary gift of genius. It is character and not talent which is the essential element of success in science. But as the muscle of the smith grows stronger by reason of its constant use in hammering, so it seems impossible to doubt that the training of the moral faculty, necessarily undergone by the philosopher, must react upon the man. There are, indeed, lamentable examples of men who seem to have one moral faculty for science, and another for their daily affairs: but such instances are hardly found in the highest ranks of philosophy; and when they occur, the daily poison may be traced spreading higher and higher, and sooner or later falling like a Nemesis upon the scientific faculty.

Let those who doubt the efficacy of science as moral discipline, make the experiment of trying to come to a comprehension of the meanest worm or weed, of its structure, its habits, its relation to the great scheme of nature. It will be a most exceptional case, if the mere endeavour to give a correct outline of its form, or to describe its appearance with accuracy, do not call into exercise far more patience, perseverance, and self-denial than they have easily at command; and if they do not rise up from the attempt, in utter astonishment at the habitual laxity and inaccuracy of their mental processes, and in some dismay at the pertinacious manner in which their subjective conceptions and hasty preconceived notions interfere with their forming a truthful conception of objective fact. There is not one person in fifty whose habits of mind are sufficiently accurate to enable him to give a truthful description of the exterior of a rose.

Finally, the *power* of natural history was illustrated by examples of recent applications of that science in opening up sources of industrial wealth.

[T. H. H.]

WEEKLY EVENING MEETING,

Friday, February 22.

Sir Benjamin Collins Brodie, Bart. D.C.L. F.R.S.
Vice-President, in the Chair.

Mr. Faraday, D.C.L. F.R.S.

On certain Magnetic Actions and Affections.

All bodies subject to magnetic induction, when placed in the ordinary magnetic field between the poles of a magnet, are affected; paramagnetic bodies tend to pass bodily from weaker to stronger places of force, and diamagnetic bodies from stronger to weaker places of force. If the bodies are elongated, then those that are paramagnetic set along the lines of force, and those that are diamagnetic across them: but if these bodies have a spherical form, are amorphous, and are perfectly free from permanent magnetic charge, they have no tendency to set in a particular direction. Nevertheless, there are bodies of both classes, which being *crystalline*, have the power of setting when a single crystal is wrought into the form of a sphere, and these are called magne-crystals; their number is increasing continually; carbonate of lime, bismuth, tourmaline, &c., are of this nature. Bodies which being magnetic, set, because they are elongated, are greatly influenced in the force of the set by the nature of the medium surrounding them, and to such an extent that they not merely vary in their force from the maximum to nothing, but will often set axially in one medium, and equatorially in another. Yet the same bodies, if magne-crystallic and formed into spheres, though they set well in the magnetic field, will set with the *same* force whatever the change in the media about them, and are perfectly freed from the influence of the latter. Thus, if a crystal of bismuth formed into a sphere, or a vertical cylinder, has, when suspended, its magne-crystallic axis horizontal, and if the various media about it, from saturated solution of sulphate of iron, up to phosphorus, through air, water, alcohol, oil, be changed one for another, no alteration in the amount of torsion force required to displace the magne-crystal will occur, provided the force of the magnet be constant, notwithstanding that the list of media includes highly paramagnetic and diamagnetic bodies; and in such cases the measurement of the power of set is relieved from a multitude of interfering circumstances existing in other cases, and *that power* which is dependent upon the internal structure and con-

dition of the substance is proved to be, at the same temperature, always the same.

A consequence of magne-crystallic structure is that the same body is more paramagnetic, or more diamagnetic in one direction than in another; and therefore it follows, that though such a crystal may have no variation in set-force, produced by change of the surrounding medium, it may have a variation produced in the absolute force of attraction or repulsion; even up to the point of being attracted in one position and repelled in another, though no change in form, or in the surrounding medium, or in the force of the magnet, or in the nature of the body itself, be made, but simply a change in the *direction of the structure*. This was shown by a crystal of the red ferroprussiate of potassa, which, being coated carefully with wax, was suspended from the arm of a torsion balance so that it dipped into a solution of proto-sulphate of iron occupying the magnetic field.* When the magne-crystallic axis was parallel to the lines of force the crystal was attracted by the magnetic pole, when it was perpendicular to the lines of force the crystal was repelled; acting like a paramagnetic and a diamagnetic in turns. No magne-crystal has yet been found having such a relation to a vacuum, or to carbonic acid (its magnetic equivalent); calcareous spar is nearly coincident with such a medium, and shows different degrees of force in the two directions, but is always a little on the diamagnetic side. Calcareous spar having a trace of iron has been found very nearly up to the desired point, on the paramagnetic side; and as these preserve the full magne-crystallic relation of the two directions, there is no reason to suppose that a crystal may not be found which may not be paramagnetic in one direction, and diamagnetic in another, in respect of space as zero.

There is every reason to believe that the general magnetic relations of a magne-crystal are the same with those of the same substance in the amorphous state; and that the circumstances which influence one, influence the other to the same degree. In that case, the magnetic affections of a body might be ascertained by the examination of the magne-crystallic affections; thus the effect of heat upon bismuth, tourmaline, &c., might be examined by the set of the crystals; and with so much the greater advantage, that short globular forms could be used, perfectly free from the magnetic influence of the surrounding media required as temperature baths, and requiring no displacement of these media with the motion of the crystal. So crystals of bismuth, tourmaline, carbonate of iron, and other bodies, were suspended in baths of oil, water, &c., the temperature gradually raised and lowered, and the torsion force of the set for each temperature observed. With bismuth, a crystal having a force of 200 at 20° F. was reduced to a force of 70 at 300°, and the diminution of force appeared to be nearly equal in all parts of the scale for an

* $2\frac{1}{2}$ volumes of saturated solution, at 65° F., and 1 volume of water.

equal number of degrees. A piece of amorphous bismuth, compressed in one direction, gave nearly the same amount and degree of change for the same alteration of temperature; leading us to the persuasion that the whole magnetic force of bismuth as a diamagnetic body would suffer like change. A crystal of tourmaline, which at 0° had a setting force of 540, when raised to 300°, had a setting force of only 270 : the loss of force was progressive, being greater at lower than at high temperatures; for a change from 0° to 30° caused a loss of force equal 50, whilst a change from 270° to 300°, caused a loss of only 20. Carbonate of iron suffered a like change; at 0° the force was 1140, at 300° it was only 415; at the lower temperature the loss for 30° was 120 of force, at the upper it was only 34.

In all these and in many other cases, both with paramagnetic and diamagnetic bodies, the magne-crystallic differences diminished with the elevation of temperature; and therefore it may be considered probable, that the actual magnetic force changed in the same direction. But on extending the results to iron, nickel, and cobalt, employing these metals as very small prisms associated with copper cubes to give them weight, it was found that another result occurred. Iron, whether at the temperature of 30° or 300°, or any intermediate degree, underwent no change of force, it remained at 300, which was the expression for the piece employed under the circumstances. We know that at higher temperatures it loses power, and that at a bright red it is almost destitute of inductive magnetic force. A piece of nickel, which at 95° had a setting power of 300, when raised to 285°, had a power of only 290, so that it had lost a thirtieth part of its force; at the heat of boiling oil, it is known to lose nearly all its force, being unable then to affect a magnetic needle. Cobalt, on the other hand, requires a far higher temperature than iron to remove its magnetic character, a heat near that of melting copper being necessary. As to lower temperatures it was found, that an elevation from 70° to 300° caused an absolute *increase* of the magnetic force from 293 to 333. It is evident, therefore, that there is a certain temperature, or range of temperature above 300°, at which the magnetic force of cobalt is a maximum; and that elevation above, or depression below that temperature causes a diminution of the force. The case is probably the same for *iron;* its maximum magnetic force occurring at temperatures between 0° and 300°. If nickel is subject to the same conditions of a maximum, then that state must come on at temperatures below 0°: and it may be further remarked, that as the maximum conditions occur in the following order for ascending temperatures, nickel, iron, cobalt, such also is the same order for the temperatures at which they lose their high and distinctive magnetic place amongst metals.

[M. F.]

WEEKLY EVENING MEETING,

Friday, February 29.

SIR HENRY HOLLAND, M.D. F.R.S. Vice-President,
in the Chair.

PROFESSOR WM. THOMSON, F.R.S.

On the Origin and Transformations of Motive Power.

THE speaker commenced by referring to the term *work done*, as applied to the action of a force pressing against a body which yields, and, to the term *mechanical effect produced*, which may be either applied to a resisting force overcome, or to matter set in motion. Often the mechanical effect of work done consists in a combination of those two classes of effects. It was pointed out that a careful study of nature leads to no firmer conviction than that work cannot be done without producing an indestructible equivalent of mechanical effect. Various familiar instances of an apparent loss of mechanical effect, as in the friction, impact, cutting, or bending of solids, were alluded to, but especially that which is presented by a fluid in motion. Although in hammering solids, or in forcing solids to slide against one another, it may have been supposed that the alterations which the solids experience from such processes constitute the effects mechanically equivalent to the work spent, no such explanation can be contemplated for the case of work spent in agitating a fluid. If water in a basin be stirred round and left revolving, after a few minutes it may be observed to have lost all sensible or otherwise discernible signs of motion. Yet it has not communicated motion to other matter round it; and it appears as if it has retained no effect whatever from the state of motion in which it had been. It is not tolerable to suppose that its motion can have come to nothing; and until fourteen years ago confession of ignorance and expectation of light was all that philosophy taught regarding the vast class of natural phenomena, of which the case alluded to is an example. Mayer, in 1842, and Joule, in 1843, asserted that heat is the equivalent obtained for work spent in agitating a fluid, and both gave good reasons in support of their assertion. Many observations have been cited to prove that heat is not generated by the friction of fluids: but that heat is generated by the friction of fluids has been established beyond all doubt by the powerful and refined tests applied by Joule in his experimental investigation of the subject.

An instrument was exhibited, by means of which the temperature of a small quantity of water, contained in a shallow circular case provided with vanes in its top and bottom, and violently agitated by a circular disc provided with similar vanes, and made to turn rapidly round, could easily be raised in temperature several degrees in a few minutes by the power of a man, and by means of which steam power applied to turn the disc had raised the temperature of the water by 30° in half an hour. The bearings of the shaft, to the end of which the disc was attached, were entirely external; so that there was no friction of solids under the water, and no way of accounting for the heat developed except by the friction in the fluid itself.

It was pointed out that the heat thus obtained is not *produced from a source*, but is *generated;* and that what is called into existence by the work of a man's arm cannot be matter.

Davy's experiment, in which two pieces of ice were melted by rubbing them together in an atmosphere below the freezing point, was referred to as the first completed experimental demonstration of the immateriality of heat, although not so simple a demonstration as Joule's; and although Davy himself gives only defective reasoning to establish the true conclusion which he draws from it. Rumford's inquiry concerning the "Source of the Heat which is excited by Friction" was referred to as only wanting an easy additional experiment—a comparison of the thermal effects of dissolving (in an acid for instance), or of burning, the powder obtained by rubbing together solids, with the thermal effects obtained by dissolving or burning an equal weight of the same substance or substances in one mass or in large fragments—to prove that the heat developed by the friction is not *produced from the solids*, but is *called into existence between them.* An unfortunate use of the word "capacity for heat," which has been the occasion of much confusion ever since the discovery of latent heat, and has frequently obstructed the natural course of reasoning on thermal and thermo-dynamic phenomena, appears to have led both Rumford and Davy to give reasoning which no one could for a moment feel to be conclusive, and to have prevented each from giving a demonstration which would have established once and for ever the immateriality of heat.

Another case of apparent loss of work, well known to an audience in the Royal Institution—that in which a mass of copper is compelled to move in the neighbourhood of a magnet—was adduced; and an experiment was made to demonstrate that in it also heat appears as an effect of the work which has been spent. A copper ball, about an inch in diameter, was forced to rotate rapidly between the poles of a powerful electro-magnet. After about a minute it was found by a thermometer to have risen by 15° Fahr. After the rotation was continued for a few minutes more, and again stopped, the ball was found to be so hot that a piece of phosphorus *applied to any* point of its surface immediately took fire. It is

clear that in this experiment the electric currents, discovered by Faraday to be induced in the copper in virtue of its motion in the neighbourhood of the magnet, generated the heat which became sensible. Joule first raised the question, Is any heat generated by an induced electric current in the locality of the inductive action? He not only made experiments which established an affirmative answer to that question, but he used the mode of generating heat by mechanical work established by those experiments, as a way of finding the numerical relation between units of heat and units of work, and so first arrived at a determination of the mechanical value of heat. At the same time (1843) he gave another determination founded on the friction of fluids in motion; and six years later he gave the best determination yet obtained, according to which it appears that 772 foot pounds of work, (that is 772 times the amount of work required to overcome a force equal to the weight of 1 lb. through a space of 1 foot,) is required to generate as much heat as will raise the temperature of a pound of water by one degree.

The reverse transformation of heat into mechanical work was next considered, and the working of a steam-engine was referred to as an illustration. An original model of Stirling's air-engine was shown in operation, developing motive power from heat supplied to it by a spirit lamp, by means of the alternate contractions and expansions of one mass of air. Thermo-electric currents, and common mechanical action produced by them, were referred to as illustrating another very distinct class of means by which the same transformation may be effected. It was pointed out that in each case, while heat is taken in by the material arrangement or machine, from the source of heat, heat is always given out in another locality, which is at a lower temperature than the locality at which heat is taken in. But it was remarked that the quantity of heat given out is not, (as Carnot pointed out, it would be if heat were a substance,) the same as the quantity of heat taken in, but, as Joule insisted, less than the quantity taken in by an amount mechanically equivalent to the motive power developed. The modification of Carnot's theory to adapt it to this truth was alluded to; and the great distinction which it leads to between reversible and not reversible transformations of motive power was only mentioned.

To facilitate farther statements regarding transformations of motive power, certain terms, introduced to designate various forms under which it is manifested, were explained. Any piece of matter, or any group of bodies, however connected, which either is in motion, or can get into motion without external assistance, has what is called mechanical energy. The energy of motion may be called either "dynamical energy," or "actual energy." The energy of a material system at rest, in virtue of which it can get into motion, is called "potential energy," or, generally, motive power possessed among different pieces of matter, in virtue of their relative

positions, is called potential energy. To show the use of these terms, and explain the ideas of a *store of energy*, and of conversions and transformations of energy, various illustrations were adduced. A stone at a height, or an elevated reservoir of water, has potential energy. If the stone be let fall, its potential energy is converted into actual energy during its descent, exists entirely as the actual energy of its own motion at the instant before it strikes, and is transformed into heat at the moment of coming to rest on the ground. If the water flow down by a gradual channel, its potential energy is gradually converted into heat by fluid friction, and the fluid becomes warmer by a degree Fahr. for every 772 feet of the descent. There is potential energy, and there is dynamical energy, between the earth and the sun. There is most potential energy and least actual energy in July, when they are at their greatest distance asunder, and when their relative motion is slowest. There is least potential energy and most dynamical energy in January, when they are at their least distance, and when their relative motion is most rapid. The gain of dynamical energy from the one time to the other is equal to the loss of potential energy.

Potential energy of gravitation is possessed by every two pieces of matter at a distance from one another; but there is also potential energy in the mutual action of contiguous particles in a spring when bent, or in an elastic cord when stretched.

There is potential energy of electric force in any distribution of electricity, or among any group of electrified bodies. There is potential energy of magnetic force between the different parts of a steel magnet, or between different steel magnets, or between a magnet and a body of any substance of either paramagnetic or diamagnetic inductive capacity. There is potential energy of chemical force between any two substances which have what is called affinity for one another,—for instance, between fuel and oxygen, between food and oxygen, between zinc in a galvanic battery and oxygen. There is potential energy of chemical force among the different ingredients of gunpowder or gun cotton. There is potential energy of what may be called chemical force, among the particles of soft phosphorus, which is spent in the allotropic transformation into red phosphorus; and among the particles of prismatically crystallized sulphur, which is spent when the substance assumes the octahedral crystallization.

To make chemical combination take place without generating its equivalent of heat, all that is necessary is to resist the chemical force operating in the combination, and take up its effect in some other form of energy than heat. In a series of admirable researches on the agency of electricity in transformations of energy,* Joule

* On the Production of Heat by Voltaic Electricity," communicated to the Royal Society Dec. 17, 1840, (*see* Proceedings of that date,) and published *Phil. Mag. Oct. 1841.*

[On

showed that the chemical combinations taking place in a galvanic battery may be directed to produce a large, probably in some forms of battery an unlimited, proportion of their heat, not in the locality of combination, but in a metallic wire at any distance from that locality; or that they may be directed not to generate that part of their heat at all, but instead to raise weights, by means of a rotating engine driven by the current. Thus if we allow zinc to combine with oxygen by the beautiful process which Grove has given in his battery, we find developed in a wire connecting the two poles the heat which would have appeared directly if the zinc had been burned in oxygen gas; or if we make the current drive a galvanic engine, we have, in weights raised, an equivalent of potential energy for the potential energy between zinc and oxygen spent in the combination.

The economic relations between the electric and the thermodynamic method of transformation from chemical affinity to available motive power were indicated, in accordance with the limited capability of heat to be transformed into potential energy, which the modification of Carnot's principle, previously alluded to, shows, and the unlimited performance of a galvanic engine in raising weights to the full equivalent of chemical force used, which Joule has established.

The transformation of motive power into light, which takes place when work is spent in an extremely concentrated generation of heat, was referred to. It was illustrated by the ignition of platinum wire by means of an electric current driven through it by the chemical force between zinc and oxygen in the galvanic battery; and by the ignition and volatilization of a silver wire by an electric current driven through it by the potential energy laid up in a Leyden battery, when charged by an electrical machine. The luminous heat generated in the last-mentioned case was the complement to a deficiency of heat of friction in the plate-glass and

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"On the Heat evolved by Metallic Conductors of Electricity, and in the cells of a battery during Electrolysis."—*Phil. Mag.* Oct. 1841.

"On the Electrical Origin of the Heat of Combustion."—*Phil. Mag.* March 1843.

"On the Heat evolved during the Electrolysis of Water," Proceedings of the Literary and Philosophical Society of Manchester, 1843, Vol. vii. Part 3, Second Series.

"On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat," communicated to the British Association (Cork), Aug. 1843, and published *Phil. Mag.* Oct. 1843.

"On the Intermittent Character of the Voltaic Current in certain cases of Electrolysis, and on the Intensity of various Voltaic arrangements."—*Phil. Mag.* Feb. 1844.

"On the Mechanical Powers of Electro-Magnetism, Steam, and Horses." By Joule and Scoresby.—*Phil. Mag.* June 1846.

"On the Heat disengaged in Chemical Combination."—*Phil. Mag.* June 1852.

"On the Economical Production of Mechanical Effect from Chemical Forces."—*Phil. Mag.* Jan. 1853.

rubber of the machine, which a perfect determination, and comparison with the amount of work spent in turning the machine, would certainly have detected.

The application of mechanical principles to the mechanical actions of living creatures was pointed out. It appears certain, from the most careful physiological researches, that a living animal has not the power of originating mechanical energy; and that all the work done by a living animal in the course of its life, and all the heat that has been emitted from it, together with the heat that would be obtained by burning the combustible matter which has been lost from its body during its life, and by burning its body after death, make up together an exact equivalent to the heat that would be obtained by burning as much food as it has used during its life, and an amount of fuel that would generate as much heat as its body if burned immediately after birth.

On the other hand, the dynamical energy of luminiferous vibrations was referred to as the mechanical power allotted to plants (not mushrooms or funguses, which can grow in the dark, are nourished by organic food like animals, and like animals absorb oxygen and exhale carbonic acid,) to enable them to draw carbon from carbonic acid, and hydrogen from water.

In conclusion, the sources available to man for the production of mechanical effect were examined and traced to the sun's heat and the rotation of the earth round its axis.

Published speculations* were referred to, by which it is shown to be possible that the motions of the earth and of the heavenly bodies, and the heat of the sun, may all be due to gravitation; or, *that the potential energy of gravitation may be in reality the ultimate created antecedent of all motion, heat, and light at present existing in the universe.**

[W. T.]

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* Trans. Roy. Soc. Edinburgh, April 1854 (Professor W. Thomson, "On the Mechanical Energies of the Solar System"). Also British Association Report, Liverpool, Sept. 1854 ("On the Mechanical Antecedents of Motion, Heat, and Light).

GENERAL MONTHLY MEETING,

Monday, March 3.

SIR B. C. BRODIE, Bart. D.C.L. F.R.S. Vice-President, in the Chair.

The Lord Stanley, M.P.
Hon. Mr. Baron Bramwell.
Rev. Charles John Fynes Clinton, M.A.
Rev. John Craig, M.A.
Edmund Beckett Denison, Esq. M.A. Q.C.
William Dodsworth, Esq.
Francis B. Duppa, Esq.
Graham M. M. Esmeade, Esq.
John Joseph Forrester, Esq.
Ralph Allen Husey, Esq.
Alexander Murray, Esq.
Francis Pitney Brouncker Martin, Esq. M.A. and
John Pyle, Esq. F.R.C.S.Eng.

were *elected* Members of the Royal Institution.

George Busk, Esq. F.R S. and
William Baker Taylor, Esq.

were *admitted* Members of the Royal Institution.

The Secretary reported that the following Arrangements had been made for the *Lectures* AFTER *Easter*, 1856:—

Four Lectures (*in continuation*) on PHYSIOLOGY AND COMPARATIVE ANATOMY, by THOMAS HENRY HUXLEY, Esq. F.R.S. Fullerian Professor of Physiology, R.I.

Seven Lectures on PHOTOGRAPHY, by T. A. MALONE, Esq. F.C.S. Director of the Laboratory, London Institution.

Eleven Lectures on LIGHT, by JOHN TYNDALL, Esq F.R.S. Professor of Natural Philosophy, R.I.

Eleven Lectures on the NON-METALLIC ELEMENTS, THEIR MANUFACTURE AND APPLICATION, by DR. A. W. HOFMANN, F.R.S.

The following PRESENTS were announced, and the thanks of the Members were returned for the same:—

FROM—

Agricultural Society, Royal—Journal, Vol. XVI. Part 2. 8vo. 1856.

Anderson, Eustace, Esq. M.R.I. (*the Author*)—Chamouni and Mont Blanc: a Visit to the Valley and an Ascent of the Mountain in the Autumn of 1855. 16mo. 1856.

Arnold, T. J. Esq. Life-Sub. R.I.—Annual Reports of the Poor Law Commissioners and Poor Law Board, 1834-54. 21 vols. 8vo.
Report on the further Amendment of the Poor Laws, and on the continuance of the Commission. 8vo. 1840.
Report of the Poor Law Commissioners on Local Taxation. 8vo. 1844.
Asiatic Society of Bengal—Journal, No. 251. 8vo. 1855.
Astronomical Society, Royal—Monthly Notices. Vol. XVI. No. 3. 8vo. 1856.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for March, 1856. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for March, 1856. 4to.
Bradbury, Henry, Esq. M.R.I.—The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Part 11. fol. 1856.
British Architects, Royal Institute of—Proceedings in Feb. 1856. 4to.
British and Foreign Bible Society—Reports, Vol. XVI. and XVII. 1849-54.
The Holy Bible in Lithuanian. 8vo. 1853.
The Old Testament in Hindustani, and in Turco-Greek. 3 vols.
The New Testament in Chinese, Feejean, Canarese, Tongan, French Basque, Kaffir, Maltese, and Modern Greek and Albanian. 8 vols.
Parts of the Scriptures in Accra (or Ga), Cree, Hinduï, Kutchee, Kinika, Mahratta, Micmac, Khassee, Punjabi, Scinde, and Yoruba: in 12 vols.
Civil Engineers, Institute of—Proceedings in Feb. 1856. 8vo.
De Romand, Baron G. (the Author)—Un Mot sur le Caractère et les conséquences de la Paix Future. 8vo. Paris, 1856.
Devincenzi, M. J. (the Author)—Mémoire sur l'Electrographie. 4to. 1856.
Editors—The Medical Circular for Feb. 1856. 8vo.
The Practical Mechanic's Journal for Feb. 1856. 4to.
The Journal of Gas-Lighting for Feb. 1856. 4to.
The Mechanic's Magazine for Feb. 1856. 8vo.
The Athenæum for Feb. 1856. 4to.
The Engineer for Feb. 1856. 4to.
Newton's London Journal, March 1856. 8vo.
Faraday, Professor, D.C.L F.R.S.—Kaiserliche Akademie, Wien:
Phil.-Hist. Classe—Sitzungsberichte. Band XVI. Heft 2; Band XVII. Heft 1. 8vo. 1855.
Monumenta Habsburgica: 1ste Abtheilung. Band II. 8vo. 1855.
Fontes Rerum Austriacarum: Erste Abtheilung, Scriptores. Zweite Abtheilung, Acta, Band VIII. IX. 8vo. 1855.
Notizenblatt für 1855. Nos. 13-24. 8vo.
Archiv. Band XIV. Heft 2. Band XV. Heft 1. 8vo. 1855.
Math.-Nat. Classe—Sitzungsberichte. Band XVI. Heft 2. Band XVII. 8vo. 1855.
Monatsberichte der Königl. Preuss. Akademie, Dec. 1855. 8vo. Berlin.
Franklin Institute of Pennsylvania—Journal, Vol. XXXI. No. 1. 8vo. 1855.
Graham, George, Esq. (Registrar-General)—Report of the Registrar-General for Feb. 1856. 8vo.
J. P. Esq.—Asphyxia, its Nature and Remedy. By Marshall Hall, M.D. 8vo. 1856.
Linnean Society of London—Journal of Proceedings. Vol. I. No. 1. 8vo. 1856.
Macrory, Edmund, Esq. M.R.I.—W. Crawford, History of Ireland from the Earliest Period to the Present Time. 2 vols. 8vo 1783.
D. Taaffe, Impartial History of Ireland from the English Invasion to the Present Time. 4 vols. 8vo. 1809-11.
S. O'Halloran, History of Ireland. 3 vols. 8vo. 1819.
Novello, Mr. (the Publisher)—The Musical Times, for Feb. 1856. 4to.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie. Heft 12. 4to. Gotha, 1855.
Philological Society—Transactions for 1855. Nos. 12 and 13. 8vo.
Photographic Society—Journal, No. 39. 8vo. 1856.
Royal Society of London—Proceedings. Nos. 17, 18. 8vo. 1855-6.
Society of Arts.—Journal for Feb. 1856. 8vo.

Taylor, Rev. W. F.R.S. M.R.I.—Magazine for the Blind. No. 17. 4to. 1855.
Thomas, Rev. Vaughan (the Author)—Memoir of the Rev. Samuel Warneford, LL D. 8vo. 1855.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Nov. und Dec. 1855. 4to. Berlin.
Vincent, B. Assist. Sec. R.I.—Tracts by Bps. Burnett and Fowler, and by W. Whiston, &c. 1 vol. 8vo. 1719-31.
Yates, James, Esq. F.R.S. M.R.I. (the Author)—Narrative of the Origin and Formation of the International Association for obtaining a uniform Decimal System of Measures, Weights, and Coins. 8vo. 1856.

WEEKLY EVENING MEETING,

Friday, March 7, 1856.

Sir B. C. Brodie, Bart. D.C.L. F.R.S. Vice-President, in the Chair.

Sir Charles Lyell, F.R.S.

On the successive Changes of the Temple of Serapis.

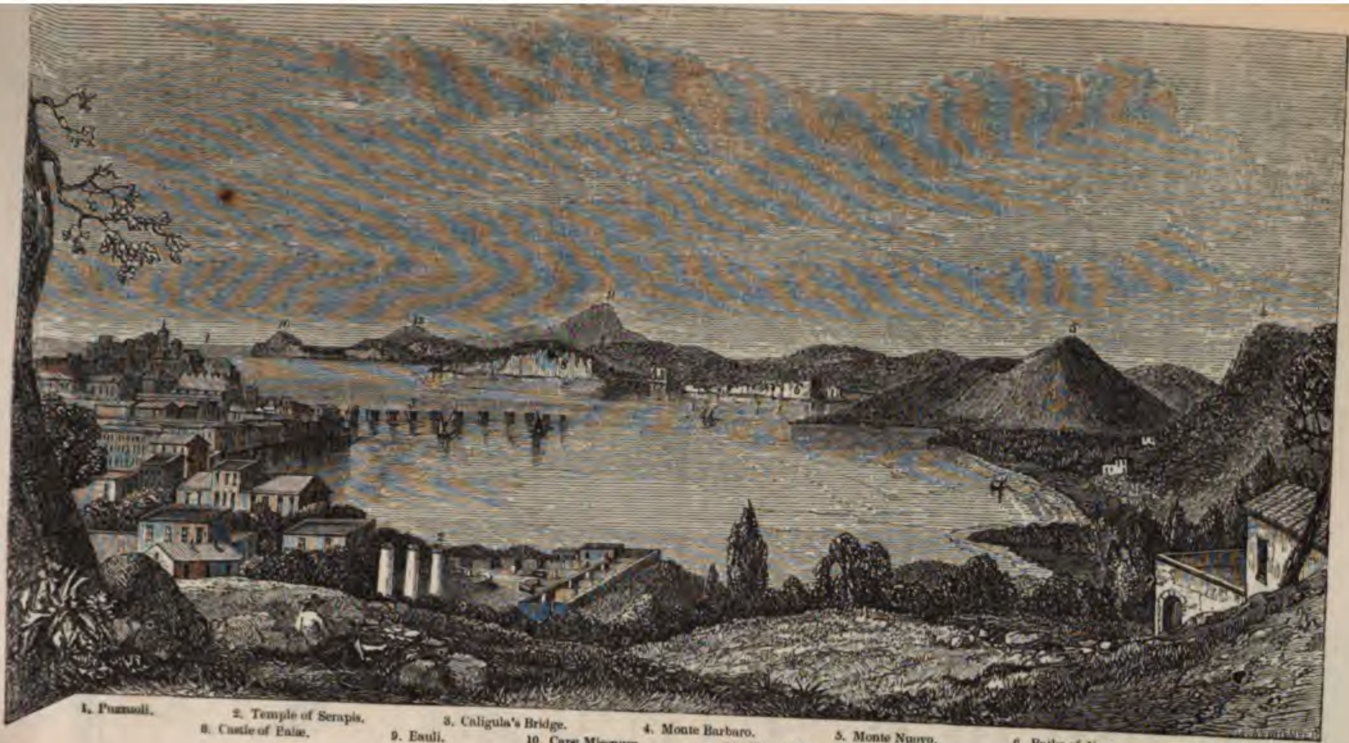
The Temple of Serapis, near Naples, is, perhaps, of all the structures raised by the hands of man, the one which affords most instruction to a geologist. It has not only undergone a wonderful succession of changes in past time, but is still undergoing changes of condition, so that it is ever a matter of fresh interest to learn what may be the present state of the temple, and to speculate on what next may happen to it. This edifice was exhumed in 1750, from a mixed deposit, extending for miles along the eastern shores of the Bay of Baiæ, and consisting partly of strata containing marine shells, with fragments of bricks, pottery, and sculpture, and partly of volcanic matter of subaerial origin. Various theories were proposed in the last century to explain the lithodomous perforations, and attached serpulæ, observed on the middle zone of the three erect marble columns now standing; some writers, and the celebrated Goethe among the rest, suggesting that a lagoon had once existed in the atrium, filled, during a temporary incursion of the sea, with salt water, and that marine mollusca and annelids flourished for years in this lagoon, at a height of 12 feet or more above the sea level. This hypothesis was advanced at a time when almost any amount of fluctuation in the level of the sea was thought more probable than the slightest alteration in the level of the solid land. In 1807, the architect Niccolini observed that the pavement of the temple was dry, except when a violent south wind was blowing; whereas, on revisiting the temple 15 years later, he found the pavement covered by salt water twice every day at high tide. This induced him to make a series of measurements from year to year,

first from 1822 to 1838, and afterwards from 1838 to 1845; from which he inferred that the sea was gaining annually upon the floor of the temple, at the rate of about one-third of an inch during the first period, and about three-fourths of an inch during the second. Mr. Smith, of Jordan-hill, when he visited the temple in 1819, had remarked that the pavement was then dry, but that certain channels cut in it for draining off the waters of a hot spring, were filled with sea water. On his return, in 1845, he found the high-water mark to be 28 inches above the pavement, which, allowing a slight deduction on account of the tide, exhibited an average rise of about an inch annually. As these measurements are in accordance with others, made by Mr. Babbage in 1828, and by Professor James Forbes, in 1826 and 1843, Mr. Smith believes his own conclusion to be nearest the truth, and attributes the difference between his average and that obtained by Niccolini (especially in the first set of measurements by the latter observer), to the rejection by the Italian architect, of all the highest water-marks of each year, causing his mean to be below the true mean level of the sea. In 1852, Signor Arcangelo Scacchi, at the request of Sir Charles Lyell, visited the temple, and compared the depth of water on the pavement with its level as previously ascertained by himself in 1839, and found, after making allowance for the tide at the two periods, that the water had gained only 4½ inches in thirteen years, and was not so deep as when measured by M.M. Niccolini and Smith, in 1845; from which he inferred, that after 1845, the downward movement of the land had ceased, and before 1852, had been converted into an upward movement. Since that period, no exact account of the level of the water seems to have been taken, or at least none which has been published.

Sir Charles Lyell then called attention to the head of a statue, lent to him for exhibition by Mr. W. R. Hamilton, and which Mr. H. had purchased from a peasant at Puzzuoli, in the neighbourhood of the temple. This head bears all the distinctive marks of the Jupiter Serapis of the Vatican; and, among others, a flat space is seen on the crown, doubtless intended to receive the ornament, called the modius, or bushel, an emblem of fertility, which adorns the ancient representations of this deity. One side of the head is uninjured, as if it had lain in mud or sand, while the other has "suffered a sea change," having been drilled by small annelids, and covered with adhering serpulæ, as if submerged for years in salt water, like the three marble columns before mentioned.

The speaker then alluded to an ancient mosaic pavement, found at the time of his examination of the temple, in 1828, five feet below the present floor, implying the existence of an older building before the second temple was erected. The latter is ascertained by inscriptions, found in the interior, to have been built at the close of the second and beginning of the third centuries of the Christian era.

A brief chronological sketch was then given of the series of



1. Puzzuoli. 2. Temple of Serapis. 3. Caligula's Bridge. 4. Monte Barbaro. 5. Monte Nuovo. 6. Baths of Nero. 7. Baiæ. 8. Castle of Baiæ. 9. Bauli. 10. Cape Misenum. 11. Mount Epomeo in Ischia. 12. South part of Ischia

TEMPLE OF SERAPIS—BAY OF BALÆ.

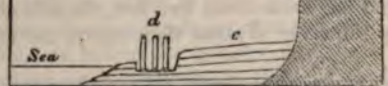
natural and historical events connected with the temple and the surrounding region; comprising the volcanic eruptions of Ischia, Monte Nuovo, and Vesuvius; the date of the first and second temples, and their original height above the sea; the periods of the submergence and emergence of the second temple; the nature of the submarine and supramarine formations, in which it was found buried in 1750; and, lastly, allusion was made to a bird's-eye view of this region, published at Rome in 1652, and cited by Mr. Smith, in which the three columns are represented as standing in a garden, at a considerable distance from the sea, and between them and the sea two churches, occupying ground which has since disappeared. The history of the sinking and burying of the temple in the dark ages, respecting which no human records are extant, has been deduced from minute investigations made by Mr. Babbage and Sir Edmund Head, in 1828, respecting the nature and contents of certain deposits formed round the columns, below the zone of lithodomous perforations.

Fig 1.

TEMPLE OF SERAPIS at its period of greatest depression, between A.D. 1000 and A.D. 1500.*
a, b, Ancient mosaic movement. *c c*, Dark marine incrustation. *d d*, First filling up, shower of ashes. *e e*, Fresh water calcareous deposit. *f f*, Second filling up. A, Stadium.

The unequal amount of movement in the land and bed of the sea, and its different directions in adjoining areas in and around the Bay of Baiæ, were then pointed out; and the fact that the Temples of Neptune and the Nymphs are now under water, as well as some

Fig. 2.



a, Remains of Cicero's villa, N. side of Puzzuoli. *b*, Ancient cliff, now inland.
c, Terrace (called La Starza) composed of recent submarine deposits.
d, Temple of Serapis in 1845, 2 ft. 4 in. below sea-level.

* A detailed account of the several up-fillings of the Temple represented in this cut will be found in the 9th Edition of *The Principles of Geology* (1853), p. 514.

Roman roads, while no evidence of any corresponding subsidence or oscillations of level are discoverable on the site of the city of Naples, which is only four miles distant in a straight line. Analogous examples of upward and downward movements in other parts of the Mediterranean were cited, such as the sarcophagus of Telmessus in Lycia, described by Sir Charles Fellows; and the changes in Candia, recently established by Captain Spratt, R.N., who has ascertained that the western end of that island has been uplifted 17 feet above its ancient level, while another part of the southern coast has risen more than 27 feet, so that the docks of ancient Grecian ports are upraised, as well as limestone rocks drilled by lithodomi. At the same time the eastern portion of Candia (an island about 200 miles long,) has sunk many feet, causing the ruins of several Greek towns to be visible under water. Looking beyond the limits of the Mediterranean, the buried Hindoo temple of Avantipura in Cashmere, with its 74 pillars, described by Dr. Thomson and Major Cunningham, were mentioned, and how their envelopment in lacustrine silt, at some period after the year 850 of our era, had caused them and their statues to escape the fury of the Mahometan conqueror Sicander, who bore the nam- of the idol-breaker. (*Principles of Geology*, *9th edition*, p. 762.) The gradual subsidence of the coast of Greenland, and the elevation of a large part of Sweden, century after century, were also instanced; and lastly, the latest event of the kind, yielding to no other in the magnitude of its geological and geographical importance, the earthquake of New Zealand, of January 23rd, 1855. The shocks of this convulsion extended over an area of land and sea three times as large as the British Isles; after it had ceased, it was found that a tract of land, in the immediate vicinity of Wellington, comprising 4600 square miles, or nearly equal to Yorkshire in dimensions, had been upraised from one to nine feet, and a range of hills, consisting of older rocks, uplifted vertically, while the tertiary plains to the east of it remained unmoved; so that a precipice, nine feet in perpendicular height was produced, and is even said to be traceable for 90 miles inland, from north to south bordering the plain of the Wairarapa. In consequence of a rise of five feet of the land on the north side of Cook's Strait, near Wellington and Port Nicholson, the tide had been almost excluded from the river Hutt, while on the south side of the same straits in the Middle Island, where the ground has sunk about five feet, the tide now flows several miles further up the river Wairau than before the earthquake.*

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* Some memoranda respecting the changes in physical geography, effected during the earthquake of January 23rd, 1855, will be found in the Appendix of a new work by the Rev. Richard Taylor, *entitled* "New Zealand and its Inhabitants," London, 1855. These

Sir Charles then alluded to his discovery, in 1828, of marine shells in volcanic tuff, at the height of nearly 2000 feet, in the island

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were furnished by Mr. Edward Roberts, of the Royal Engineer Department, who has since (March 1856), on his return to London, communicated other particulars to Sir C. Lyell. Mr. Walter Mantell, also now in London, and who was in Wellington (New Zealand) during the shocks of last year, besides confirming the statements of Mr. Roberts, has supplied valuable information respecting the geological structure of the country upraised or depressed during the catastrophe. The upheaval around Wellington was only from one and a half to four feet, but went on increasing gradually to Muka Muka Point, 12 miles distant, in a direct line to the south-east, where it reached its maximum, amounting to nine feet, and beyond, or eastward of which, there was no movement. Mr. Roberts was enabled to make these measurements with accuracy, as a white zone of rock, covered with nullipores just below the level of low tide, was upraised.

The perpendicular cliff, at the point above mentioned, formed part of the seaward termination of the Rimutaka chain of hills, which consist of argillite (not slaty), of ancient geological date. Their eastern escarpment faces a low country, consisting of very modern tertiary strata, which also terminate when they reach the sea in a cliff, 80 feet high, and considerably lower than that formed by the older rocks. This tertiary cliff remained absolutely unmoved, the junction of the older and newer rocks constituting a line of fault, running north and south, for a great distance (according to a resident, 90 miles,) inland along the base of the hills, where rising abruptly they bound the low tertiary plains. A fissure open in part of its course, and in which some cattle were engulphed in 1855, marks the line of fault in many places.

Among other proofs of subsidence experienced on the opposite side of Cook's Straits, or in the northern part of the Middle Island, contemporaneously with the upheaval above mentioned, Mr. Roberts states, that settlers have now to go three miles further up the river Wairau to obtain supplies of fresh water, than they did before the earthquake of January 1855. There was no volcanic eruption in the northern island at the time of these events; but the natives allege that the temperature of the Taupo hot-springs was sensibly elevated just before the catastrophe.

During a previous earthquake in 1832, other alterations in the relative level of land and sea occurred; and many of the colonists fear a repetition of such movements every seven years, for in 1841 and in 1848 there were violent convulsions. The larger part, however, of New Zealand has not suffered any injury during the same period from earthquakes.

of Ischia; and to the exact agreement of these, as well as other fossil shells, since collected by M. Philippi, with species now inhabiting the Mediterranean. If the antiquity of such elevated deposits, when contrasted with those found during the last 2000 years in the neighbourhood of the Temple of Serapis, be as great as the relative amount of movement in the two cases, or as 2000 is to 30 feet, it would show how slowly the testaceous fauna of the Mediterranean undergoes alteration: and therefore that naturalists ought not to expect to detect any sensible variation in the marine fauna in the course of a few centuries, or even several thousand years.

In conclusion: the probable causes of the permanent upheaval and subsidence of land were considered—the expansion of solid rocks by heat, and their contraction when the temperature is lowered, the shrinkage of clay when baked, the excess in the volume of melted stone over the same materials when crystallized, or in a state of consolidation; and, lastly, the subterraneous intrusion of horizontal dikes of lava, such as may have been injected beneath the surface, when melted matter rose to the crater of Monte Nuovo, in 1538. A large coloured section of a cliff, 1000 feet high, at Cape Giram, in Madeira, was referred to as illustrating the intrusion both of oblique and horizontal dikes, between layers of volcanic materials previously accumulated above the level of the sea, and after Madeira had been already clothed with a vegetation very similar to that with which it is now covered. The intercalation of such horizontal sheets of lava between alternating beds of older lava and tuff would uplift the incumbent rocks, and form a permanent support to them; but when the fused mass cools and consolidates, a partial failure of support and subsidence would ensue.

[C. L.]

WEEKLY EVENING MEETING,

Friday, March 14.

Sir B. C. Brodie, Bart. D.C.L. F.R.S. Vice-President, in the Chair.

The Rev. J. Barlow, M.A. F.R.S. V.P. Sec. R.I.

On Aluminium.

Mr. Barlow commenced his discourse by presenting M. Sainte-Claire Deville to the Members of the Institution. M. Deville had travelled from Paris for the purpose of exhibiting specimens of aluminium, both unwrought and manufactured; and also of assisting in experiments devised by him for illustrating the processes by which this remarkable metal is obtained.

Aluminium was discovered by Wöhler, in 1827.* But the means of producing this metal, to which that eminent philosopher was obliged to have recourse, were too costly to admit of its being employed in any practical use. Having succeeded in greatly diminishing this cost of production, and being supplied with funds by the liberality of the Emperor of the French, M. Deville has obtained this metal in an amount sufficient for scientific investigation and for some important applications.†

It was the object of the speaker, 1st, to cause that the difficulties attendant on the production of aluminium should be understood and appreciated: 2ndly, to describe its physical and chemical properties, and to mention uses which have been or can be made of it.

1st. For the purpose of illustrating the obstacles presented by nature to this last conquest of science, a comparison was made between the modes of producing—(*a*) *the metals of the early age;* (*b*) *of the middle age;* (*c*) *of the scientific age*, from their respective ores; and the means had recourse to for separating aluminium from its ore.

(*a*) Gold was taken as the type of the *ancient metals*. The common ore of gold is a mass of silica, to which this metal is found attached. Silica is a strong acid; but it cannot combine with gold. The two substances, therefore, being merely attracted toge-

* Ueber das Aluminium. Von F. Wöhler. Pogg. Ann. xi. 146.

† Recherches sur les Métaux. Par M. H. Sainte-Claire Deville. Annales de Chemie. Ser. 3, tom. xl. p. 21.

ther by their mutual cohesion, can be detached by mechanical means. A comparison was made between this ore of gold and pure clay, the ore of aluminium. Clay consists of silica and alumina (the oxide of aluminium). In this substance, however, the silica combines as an acid, so as to form a natural salt, from which the alumina, and then the aluminium from the alumina, have to be successively separated, not by mechanical force but by powerful chemical reagents. The ore of gold occurs rarely, but is recognized at once by any experienced eye; the ore of aluminium is one of the most abundant substances in nature, but its recognition as a metallic ore has tasked the extreme attainments of modern science.

(*b*) Iron, tin, antimony, and lead were next referred to as types of *the metals of the middle age.* Their ores were exhibited. The sapphire,* the purest form of corundum, which is native oxide of aluminium, was contrasted with hæmatite, the native sesquioxide of iron. The metallic aspect, characterizing the ores of this class of metals, was noticed. This appearance must naturally have led to these substances being subjected to the action of fire. The separation of a metal by the reducing action of the fuel of the furnace on its ore, was illustrated by metallic lead, one of the ingredients of flint glass, being made visible by the action of the flame of a spirit lamp on a piece of that substance. Glass is a combination of silica with oxide of lead and other metals; it therefore carries on the analogy with pipe-clay, the silicate of alumina: but aluminium cannot be reduced from the silicate of alumina by any known fuel, at any known temperature. Were this not the case, crucibles, furnaces, even houses which are made of clay, would be decomposed, and the aluminium they contain extracted, whenever they were exposed to the force of a sufficiently intense combustion.

(*c*) *The metals of the scientific age.*—These metals were brought into visible existence by the pile of Volta. The searching and separating properties of this wonderful invention were made known at the commencement of the present century. Simultaneously with this discovery there commenced an age of research, which began and is continued to this time in the Royal Institution. It is characterized by the historian of the "Inductive Sciences," as the epoch of Davy and of Faraday.† The knowledge and sagacity of Davy induced that distinguished man to seek for an *alkaligen* (or generator of alkali) at that (so called) pole of the voltaic battery, where Nicholson and Carlisle had recently found *hydrogen* (generator of water). This alkaligen was the metal of the alkali—the metal of the scientific age.

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* Miss Coutts, M.R.I., kindly lent three sapphires of great size and beauty to illustrate this discourse. It has been suggested that the characteristic colour of these jewels may be due to the presence of a protoxide of aluminium (Al O).

† Whewell's Hist. of Induct. Sciences, Vol. iii. p. 178.

The speaker here adverted to the enormous difference which exists, not only between the alkaline metals and those of the former groups, but also between their respective ores. Neither the ore of potassium, nor (in Davy's time) the ore of sodium, could be obtained directly from the soil through which its weak solution is diffused; it had to be sought in the tissues of plants, which absorbed it from the ground. When obtained and concentrated, this substance is fusible, soluble, corrosive, and possessed of immense chemical power. The metal derived from this strange ore, though possessing lustre, ductility, malleability, power of conducting heat and electricity, differs in every other respect from every metal yet known. Its specific gravity is less than that of water, and its affinity for oxygen so great as to necessitate the invention of hitherto unknown expedients to prevent the metal from returning to its state of oxide as soon as it was separated from it.

Davy seems to have accounted for the results he produced by assuming that the poles of the battery acted as centres of force; that the potassium being repelled by one pole was attracted by the other, like any light substance placed between two surfaces charged with opposite Franklinic electricity. Faraday* has taught that the whole of the subjected body is pervaded by this decomposing force as an axis of power, so that a particle in the middle is as much affected as a particle at the extremities of it; that, in order to be under the influence of this force, the body must be fluid, and a conductor of electricity, and that its elements must exist in a simple relation to each other, and that the proportional amount in which these elementary substances, separated by electrical decomposition, are severally ejected at the electrodes (electrical outlets), corresponds exactly with that of their combining numbers. In accordance with these views the voltaic pile may be regarded as a flameless furnace, whose reducing power is conveyed by the conducting wires to the spot where the ore is subjected to its influence. By these means not only the alkalies but the alkaline earths were decomposed, and their respective metals, barium, calcium, strontium, and lithium† obtained. But aluminium cannot be separated from alumina, its sesquioxide, by electrolysis. The metal does not in this case (as in those of potass, lime, strontia, &c.,) combine with oxygen or any like body in single proportionals; and alumina, its only known oxide, is also infusible, except before the blow-pipe.

But although the voltaic battery be unable to separate alumi-

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* Experimental Researches in Electricity, Fifth Series. A perspicuous summary of the principles of this philosophy will be found in the History of the Inductive Sciences, ed. 1837, Vol. iii. p. 163, &c.; and Grove's Correlation of the Physical Forces, ed. 1855, p. 172, &c.

† M. Warren De la Rue exhibited specimens of lithium produced from the fused chloride by means of voltaic electricity. The negative pole was an iron wire; the positive, a piece of coke. Lithium is the lightest substance known.

nium from alumina, it has effectually aided in obtaining that metal. Not only was it the first source of potassium and sodium, substances indispensable to the production of aluminium, but in the subsequent preparation of those bodies, the philosophy of the pile has rendered essential service to chemists. Faraday has shown that electrical power is identical with chemical power. This being admitted, whatever the one power can effect, the other may be expected to accomplish. This consideration sanctions, if it does not suggest, the processes by which Gay Lussac and Thénard, and Mitscherlich, and Brunner, and Donny and Mareska,* and ultimately Deville, have obtained potassium and sodium (the latter metal especially,) by direct chemical reaction; by a process so productive, that the melted sodium actually runs out in a continued stream, as in common distillation, from the iron retort, into a receptacle placed to receive it.†

The powerful deoxidizing effect of the alkaline metals has been proved by its effecting not only the decomposition of carbonic acid gas, but also by the reduction of calcium, barium, and of silicium, a substance to which the attention of the audience was especially directed.

Clay had already been designated as a silicate of alumina: in fact, three-fourths by weight of a portion of pure clay are silica. Of this silica one half is oxygen, the other half silicium, a substance altogether new in its properties; it is not affected by water or by air, and it can be kept in either; it has no lustre, or any other resemblance to a metal; it is analogous to carbon.

Now it is important to notice, that it was not from silica (the oxide), but from the fluoride and chloride of silicium that Berzelius obtained this substance. This fact perhaps instigated Wöhler's successful attempt to decompose the *chloride* of aluminium (a fusible and volatile substance,) by the vapour of potassium, which has no effect on the oxide of aluminium. But the production of the chloride of aluminium demands a concentration of chemical power. The hydrated chloride, resulting from the solution of alumina in hydrochloric acid, on being evaporated, decomposes the last portions of the mother-liquor, and the operation ends by the reproduction of alumina. This difficulty was surmounted by Œrsted: he caused the affinity of oxygen for carbon and of aluminium for chlorine to act simultaneously, and under the most favourable circumstances, by chlorine gas being led over an intimate mixture of alumina and charcoal heated to redness in a porcelain

* Recherches sur l'extraction du Potassium, par M.M. J. Mareska et F. Donny, An. de Chem. Ser. 3, tom. xxxv. p. 147.

† Recherches sur les Métaux, &c., An. de Chem., Ser. 3, tom. xliii. p. 19. M. Deville fused and cast above *half a pint* of sodium in a large ingot-mould to illustrate this part of the discourse. The present retail price of sodium, in London, is 4*s.* the ounce.

tube. The anhydrous chloride was thus evolved in vapour, and condensed in a suitable receiver. The apparatus contrived by M. Deville for procuring this substance, and described in the memoir already referred to,* was exhibited. Wöhler's process of obtaining aluminium from its chloride is well known. The following modification of that process, devised by M. Deville, was shown in action.

A tube of Bohemian glass, 36 inches long, and about one inch in diameter, was placed on an empty combustion-furnace, constructed for the purpose. Chloride of aluminium was introduced at one extremity of the tube; at the same extremity a current of dry hydrogen gas was made to enter the tube, and was sustained till the operation was finished. The chloride was now gently warmed by pieces of hot charcoal, in order to drive off any hydrochloric acid it might contain; porcelain boats, filled with sodium, were inserted into the opposite extremity of the tube; the heat was augmented by fresh pieces of glowing charcoal until the vapour of the sodium decomposed that of the chloride of aluminium. Intense ignition usually attends this re-action. At length the aluminium was liberated in buttons, which were found in the boat adhering to a substance consisting of the mixed chlorides of aluminium and sodium. The boat was now transferred, with its contents, to a porcelain tube, through which hydrogen gas was passed. At a red heat, the double chloride distilled into a receiving vessel, attached to the tube for the purpose; the buttons of aluminium were collected, washed with water, and subsequently fused together under a flux consisting of the double chloride.

Another method of obtaining aluminium from the chloride has been adopted with success. It is as follows:—

4·200 grammes of the double chloride of aluminium and sodium (*i.e.*, 2·800 grammes chloride of aluminium, and 1·400 grammes common salt),
2·100 grammes of common salt,
2·100 grammes of cryolite,

thoroughly dry, and carefully mixed together, are to be laid in alternate layers, with 840 grammes of sodium (cut into small pieces), in a crucible lined with alumina—a layer of sodium should cover the bottom of the crucible. When the crucible is filled, a little powdered salt is to be sprinkled on its contents, and the crucible, fitted with a lid, is to be introduced into a furnace, heated to redness, and kept at that temperature until a reaction, whose occurrence and continuance is indicated by a peculiar and characteristic sound, shall have terminated. The contents of the crucible, having been stirred with a porcelain rod, while in their liquified state—(this part of the operation is essential)—are poured out on a surface of

* Recherches sur les Métaux, &c.

baked clay, or any other suitable material—the flux, &c., on one side, and the metal on the other.*

In the experiment just described, the cryolite chiefly fulfils the office of a flux. But, twelve months since, Dr. Percy obtained aluminium directly from this mineral.† Cryolite is a fluoride of aluminium and of sodium. Dr. Percy found that layers of this substance, minutely pulverized, and heated with sodium in the manner described in the last experiment, yielded aluminium. Cryolite is found only in Greenland. A geological diagram of its locality, as well as some interesting specimens of the mineral itself, were exhibited by Mr. J. W. Tayler.‡

Such being the present known methods of producing this remarkable metal, the speaker adverted—

2nd. To the *properties of Aluminium.* Its physical properties are very characteristic. Its specific gravity (2·25)§ is nearly that of glass, and consequently below that of any metal (with the exception of the alkaline metals, and the metals of the alkaline earths). It is malleable, ductile, and sonorous. Its fusing point is between that of silver and zinc; it resembles silver in its excellence as a conductor of electricity. Lastly, it has great capacity for heat—(about six times that of silver). But the chemical properties of this metal are such as could not have been conceived until ascertained by experiment. Instead of reassuming oxygen (like the alkaline or earthy metals) with an energy proportioned to its extreme tenacity of that element while in the state of oxide, aluminium appears to be as indifferent to oxygen as gold or platinum are. It is not affected by sulphur, like silver; nor is it acted on (except to a very slight degree) by any of the oxy-acids in the cold, its only solvent being hydrochloric acid. The strong affinity between this metal and oxygen before its separation, contrasted with the apparently total indifference afterwards, suggests the possibility that at the instant of its coming in contact with air, aluminium may receive a fine coating of oxide—a film of transparent sapphire—from the atmosphere, which protects it against the above-named corroding substances.

This conjecture is rendered plausible by the result obtained on exposing a leaf of aluminium to the oxidizing flame of the blowpipe. The result of the combustion, though apparently a mass of alumina,

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* As time and space for fire operations in the theatre were limited, this experiment was not attempted during the discourse. But at an earlier period of the day, M. Sainte-Claire Deville had the honour of performing it, in the presence of H.R.H. Prince Albert, in the Laboratory of the Institution.

† Proceedings of the Royal Institution, Vol. ii. March 30, 1855, p. 79; Phil. Mag. Ser. 4, Vol. x. pp. 233 and 364; Comptes Rendus, Dec. 10, 1855, p. 1054.

‡ Lit. Gazette, No. 2039, p. 109.

§ The low specific gravity of aluminium, being nearly that of the flux employed in fusing it, enhances materially the difficulty of its production. M. Deville, however, melted together some pieces of pure aluminium without flux, *and cast the fused* mass in an ingot-mould before the audience.

shows by its metallic lustre, when rubbed in an agate mortar, that the oxygen has not penetrated below the surface; magnesium, on the contrary, whose oxide, like that of iron, separates immediately on being formed, when similarly treated, burns away with intense ignition.

Alloys of Aluminium may become important in the arts. The following, in the proportions given, were exhibited by Dr. Percy:—

Gold, Silver, Tin, Copper, Lead } alloyed with 5 per cent. of aluminium.

In the gold-alloy, the presence of the small proportion of aluminium was sufficient to convert the golden to a grey colour.

The copper-alloy was very interesting; it is malleable, and it "dips" of a fine golden colour. A slip of this alloy seemed to have been little affected by a fortnight's exposure to the atmosphere of the laboratory. Should this substance be found, on further trial, capable of withstanding the corroding vapours of a London atmosphere, it might be used advantageously to make "tongues" for the "reed-pipes" of organs, as a material of clock-work, &c.

Dr. Percy also exhibited other alloys of aluminium, respectively, consisting of

Tin 90—Aluminium 10
Copper 90—Aluminium 5—Tin 5
Copper 93·7—Aluminium 4·5—Silicium 1·8
Copper 80—Aluminium 20

(In this alloy the characteristic colour of copper was turned to white.)

Silver 80—Aluminium 20
(A hard and brittle alloy).

Solder of Aluminium.—Two parts of aluminium, and one of silver, will produce an aluminium solder without flux.

Uses of Aluminium.—Both the physical and the chemical properties of this metal are likely to ensure its application to many important uses, as soon as it can be supplied at a moderate price. (At present it is sold in England at £3 per ounce.)

M. Sainte-Claire Deville exhibited the beam of a balance, and weights, specially designed for the determination of small quantities, made of this metal, whose lightness peculiarly recommends it for such a purpose.

The same quality, as well as that of resisting corrosion, has been taken advantage of by the surgeon and the dentist. The

* Messrs. F. Crace Calvert and R. Johnson have made experiments on this subject.—Phil. Mag. Ser. 4, Vol. x. p. 244, &c.

sonorousness and ductility of aluminium have led to piano-forte wires being made of it; it may likewise be found useful in lining brass musical instruments, especially those having valves and slides. The property of resisting oxygen, sulphur, and acids, as well as its great power of retaining heat, render it highly valuable for culinary purposes. If, as may fairly be hoped, aluminium be hereafter produced at a rate that will bring it into competition with iron, it may supersede that metal in fabrics where lightness would, to a certain extent, compensate for inferiority of strength.

For example, the low specific gravity of aluminium, its freedom from all tendency to rust or tarnish, and its consequent power of reflecting the hot rays of the sun, indicate it as an appropriate material for the roofs of houses. As this metal is capable of plating iron, it would furnish a permanent and imperishable substitute for the paint now used for the protection of iron-railings, water-pipes, cisterns, &c., and which requires (what an aluminium surface would not) constant renewing.* The value of the oxide of aluminium in the ancient arts of life, pottery, dyeing, &c., is notorious. It may not be visionary to expect that, before this century shall have closed, equally important services in augmenting the comforts of civilized life may be performed by the metal itself.

[J. B.]

At the close of Mr. Barlow's discourse, Dr. Faraday briefly, but earnestly expressed his sense of the obligations which M. Sainte-Claire Deville had conferred on the Royal Institution by his presence on that occasion, as well as by the time, intelligence, and trouble which he had devoted to make the members and their friends thoroughly acquainted with the interesting substance so honourably associated with his name. These sentiments were cordially responded to by the company.

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* It is calculated that more than a quarter of a million sterling is annually expended, in the metropolis, on the paint necessary to protect the iron work of houses and other buildings from decay.

Royal Institution of Great Britain

WEEKLY EVENING MEETING,

Friday, April 4.

SIR HENRY HOLLAND, Bart. M.D. F.R.S. Vice-President, in the Chair.

HENRY E. ROSCOE, Esq. B.A. Ph.D.

On the Measurement of the Chemical Action of Light.

No attempt has been made, up to the present time, accurately to measure the changes brought about in chemical substances by the action of the solar rays.

The peculiar action of light on chemical bodies was first observed by Scheele on chloride of silver. Since that time the subject of the chemical action of light has attracted a large amount of attention, as the present perfection of the arts of the daguerreotypist and photographer fully testify. Although we possess so many facts concerning the chemical action of light, this branch of science has only as yet arrived at that first or qualitative stage of developement, through which every science must pass. The laws which regulate these phenomena are unknown to us, and we possess no means of accurately measuring the amount of the decomposition effected by the light.

The speaker proceeded to describe the results of a series of experiments carried on by him in conjunction with Professor Bunsen, which had for their object :—

1. To determine the laws which regulate the chemical action of light ;
2. To obtain a measure for the chemically active rays.

When aqueous solutions of chlorine, bromine, or iodine, are exposed (under certain conditions) to the direct solar rays they are decomposed, the corresponding hydracid being formed, and the oxygen of the water liberated. The difference between the amounts of free chlorine, bromine, or iodine, contained in the liquid before and after exposure to light, gives the quantity of the substance decomposed during the insolation. Now it was found that this quantity of chlorine, bromine, or iodine, which disappeared, was not proportional to the time of exposure to the light ; in twice the time, for instance, less than twice as much substance was de-

composed. The relation between the amount of light and the amount of decomposition was found in this case not to be a simple one.

This anomalous action may be explained even from a theoretical point of view. Chemical affinity is the resultant of all the forces which come into play during the reaction; hence it is not only the interchanging atoms which influence the result, but also those atoms which, without taking part in the decomposition, surround those actively engaged. The so-called catalytic phenomena show this action in a striking manner. To apply this general principle to the special case before us; we have to begin with pure chlorine water; after the first action of the light, however, hydrochloric acid is formed, hence the composition of the solution is altered, and a different result must be expected. This theoretical conclusion was verified by experiment. Chlorine water, to which 10 per cent. of hydrochloric acid was added, did not suffer any decomposition by an exposure of six hours to the direct sunlight; during which time the same chlorine water, without previous addition of hydrochloric acid, lost nearly all the free chlorine which it contained.*

In order then to obtain a true measure of the action of light on any chemical substance, it is necessary that the body formed by the decomposition should be removed from the sphere of action. This cannot be done with chlorine water; a new sensitive substance was therefore employed.

Equal volumes of chlorine and hydrogen gases when exposed to the direct sun light unite with explosion; in diffuse light, the action proceeds gradually. In presence of water the hydrochloric acid formed by the combination is immediately absorbed, and thus withdrawn from the sphere of action, and the diminution of the volume of the mixed gases arising from this absorption gives an exact measure of the amount of action effected by the light. The diminution in volume of the gas measured by the rise of water in a graduated tube was found to be regular, proving *that when the light is constant the amount of action is directly proportional to the time of exposure.*

The relation between the amount of action and the amount of light was experimentally determined, by allowing known quantities of diffuse light to fall upon the sensitive gas. Experiments thus conducted showed *that the amount of action is directly proportional to the amount or intensity of the light.* These simple relations were observed by Dr. Draper, of New York, in 1843; but his method of experimenting differed essentially from that employed in these researches, and was not susceptible of any very great degree of accuracy. The relation between the amount of action and the mass of the sensitive gas has not as yet been fully determined; experi-

* See Poggendorff's Annalen, xcvi., 373; and Quarterly Journal of Chemical Society, Oct. 1855.

ment has however already shown that the relation is not a simple one.

Many very interesting phenomena were observed in the course of these investigations. When the gas is first exposed to the light no action whatever is observed; after a short time the absorption slowly begins, and increases until a maximum has been attained, after which it proceeds regularly. This phenomenon of induction probably depends on a peculiar allotropic change which the chlorine must undergo before it is capable of uniting with the hydrogen.

The speaker concluded by expressing his intention of continuing these experiments at Heidelberg, in order exactly to determine the relation which exists between the amount of action and the volume of gas employed; to investigate the phenomenon of induction; and to obtain, if possible, an absolute measure for the chemical rays.

[H. E. R.]

GENERAL MONTHLY MEETING,

Monday, April 7.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Hon. Sir Charles Crompton, Justice Q.B.
Montague Chambers, Esq. M.P. Q.C.
Charles Palmer, Esq. and
Thomas Wilson, Esq.

were *elected* Members of the Royal Institution.

F. B. Duppa, Esq.
Graham M. M. Esmeade, Esq.
Alexander Murray, Esq.
Rev. Charles John Fynes Clinton, and
F. P. B. Martin, Esq.

were *admitted* Members of the Royal Institution.

The following Presents were announced, and the thanks of the Members returned for the same:—

From

Arnold, Thomas James, Esq. Life Sub. R.I.—Reports of the Commission on the Criminal Law. 1834–1845. fol.
The Law Amendment Journal, and Papers Nos. 1–3. Reports Nos. 1, 2. 1856.
Letter to Lord Panmure on the Magistracy. 8vo. 1856.

Astronomical Society, Royal—Monthly Notices. Vol. XVI. Nos. 4, 5. 8vo. 1856.
Author—Two Letters (on Educating the Deaf). 8vo. 1856.
Babbage, Charles, Esq. F.R.S. (the Author)—Observations (on Mr. Scheutz's Calculating Machine) at the Anniversary of the Royal Society, 1855. 8vo. 1856.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for April 1856. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for March 1856. 4to.
Botfield, Beriah, Esq. F.R.S. M.R.I. (the Author)—Some Account of the first English Bible: and of Early English Books, printed on Vellum. 4to. 1856.
Bradbury, Henry, Esq. M.R.I.—The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Part 12. fol. 1856.
British Architects, Royal Institute of—Proceedings in March 1856. 4to.
British Meteorological Society—Fourth Report read at the Annual Meeting, 1855. 8vo.
Civil Engineers, Institute of—Proceedings in March 1856. 8vo.
Cardale, John Bate, Esq. M.R.I.—The Liturgy and other Divine Offices of the Catholic Apostolic Church (Gordon Square). 8vo. 1853.
Editors—The Medical Circular for March 1856. 8vo.
The Practical Mechanic's Journal for March 1856. 4to.
The Journal of Gas-Lighting for March 1856. 4to.
The Mechanic's Magazine for March 1856. 8vo.
The Athenæum for March 1856. 4to.
The Engineer for March 1856. fol.
Faraday, Professor, D.C.L. F.R.S.—Monatsberichte der Königl. Preuss. Akademie, Jan. 1856. 8vo. Berlin.
Franklin Institute of Pennsylvania—Journal, Vol. XXXI. Nos. 2, 3. 8vo. 1856.
Geological Society—Quarterly Journal, No. 45. 8vo. 1856.
Graham, George, Esq. (Registrar-General)—Report of the Registrar-General for March 1856. 8vo.
13th, 14th, 15th, and 16th Annual Reports, for 1850–3. 8vo. 1854–6.
Granville, Rev. A. H. B. A.M.—How to settle the Church Rate Question. 8vo. 1856.
Granville, A. B. M.D. F.R.S.—Fresh Analyses of the Kissingen Mineral Waters. 1856.
Johnson, Edmund C. Esq. M.D. M.R.I.—The Life and Times of Marlborough and Wellington. By Viscount Cranborne. 8vo. 1856.
Jopling, J. Esq. (the Author)—Key to the Proportions of the Parthenon. 8vo. 1856.
Lewin, Malcolm, Esq. M.R.I. (the Author)—On Torture in Madras, &c. 8vo. 1856.
Londesborough, The Lord, K.H. M.R.I.—Miscellanea Graphica, Part 8. 4to. 1856.
Newton, Messrs.—London Journal (New Series), April 1856. 8vo.
Novello, Mr. (the Publisher)—The Musical Times, for March 1856. 4to.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1856. Heft 1. 4to. Gotha, 1856.
Photographic Society—Journal, No. 40. 8vo. 1856.
Royal Society of London—Proceedings. No. 19. 8vo. 1855-6.
Society of Arts—Journal for March 1856. 8vo.
Smith, W. A. Esq. M.R.I. (the Author)—A last Appeal! War—Peace, or an Armed Truce. 8vo. 1856.
Statistical Society—Journal, Vol. XIX. Part 1. 8vo. 1856.
Taylor, Rev. W. F.R.S. M.R.I.—Engraving and Description of Cowthorpe Oak, Yorkshire. By C. Empson. 4to. 1842.
Trustees of the British Museum—Catalogue of Shield Reptiles, with Engravings. Part 1. By J. E. Gray. 4to. 1855.
Van Diemen's Land, Royal Society of—Papers, &c. Vol. II. Part 3. 8vo. 1854.
Tasmanian Contributions to the Paris Exhibition. fol. 1855.

Webster, John, M.D. F.R.S. M.R.I.—Observations on the Medical Profession Bill. 8vo. 1856.

Wedgwood, Hensleigh, Esq. M.A. M.R.I. (the Author)—Geometry of the Three first Books of Euclid from Definitions alone. 12mo. 1856.

Anderson, Eustace, Esq. M.R.I—Piece of Granite from the Petits Mulets (near the summit of Mont Blanc), broken off during his ascent, Aug. 16, 1855.

De la Rue, Warren, Esq. M.R.I. (through Rev. J. Barlow, Sec. R.I.)—Two Specimens of Lithium, prepared from the fused Chloride, by means of Voltaic Electricity.

Moore, Mr. Josiah—Model of his Patent Ventilator.

Smith, Mr. C. H.—Two specimens of the Lizard Rock, Cornwall.

Tayler, J. W. Esq.—Two Crystals of Columbite.

Specimens of Cryolite, Sodalite, Tantalite, Sapphirin, and Allanite, from Greenland.

Taylor, Rev. W.—Taylor's Reflecting Tube.

WEEKLY EVENING MEETING,

Friday, April 11.

Sir Charles Fellows, Vice-President, in the Chair.

C. W. Siemens, Esq. C.E.

On a Regenerative Steam-Engine.

The application of the steam-engine to our various purposes of manufacture and locomotion is of very recent date, although the elastic force of steam was known even by the ancients; for we read in Hero of Alexandria, on Pneumatics (translation by Woodcroft), that the Egyptian priesthood made use of it for their somewhat undignified purpose of performing pretended miracles before an ignorant population. The first suggestion of its useful application for raising water is due to the Marquis of Worcester, and dwelt upon in his "Century of Inventions."

The idea was taken up by Papin, Savory, and Newcomen, who added important elements towards its practical realization; but to James Watt belongs the merit of having laid down a comprehensive principle of the steam-engine, and of having devised means to render the same capable of performing the rudest as well as the most delicate operations.

If any proof were wanting of the great genius of Watt, it would be sufficient to observe that the steam-engine of the present day is, in point of principle, still the same as it left his hands half a century ago, and that our age of material progress could only affect its form. Great honour, however, is due to Fulton, Stephenson, Nasmyth, and others, for having adapted the same to the most important purposes.

The steam-engine of Watt was composed of four organic parts, which were pointed out on a working model before the meeting, namely:—1. The furnace, or chamber of combustion, with its flues and chimney. 2. The boiler, or steam generator. 3. The steam-vessel, or cylinder, wherein the elastic force of the steam is imparted to the piston, or other first moving parts of the machinery. 4. The condenser, where the elastic force of the steam is destroyed by abstracting its latent heat, by injection of cold water, or by exposure of cooled metallic surfaces. In the case of high-pressure engines, it would seem that the condenser was suppressed; but it might be said, that this class of engines makes use of one great common condenser, namely the atmosphere; the separate condenser possessing only the advantage of relieving the working piston of the opposing atmospheric pressure. The only essential improvement of the steam-engine that has been introduced since the time of Watt consists in working the steam expansively, whereby a considerable economy has been attained; but it is well known that Watt foresaw the advantages that would be realised in this direction, and was prevented only by insufficiency of the mechanical means at his disposal from realising the same.

The lofty superstructure proved the soundness of the foundation Watt had laid; and it would seem hopeless to change the same, unless it could be proved that the very principle regarding the nature of heat, whereon Watt had built, had given way to another more comprehensive principle. The engine of Watt was based upon the material theory of heat that prevailed at his time, and almost to the present day. According to this theory, steam was regarded as a chemical compound of water and the supposed imponderable fluid "heat," which possessed amongst others the property of occupying under atmospheric pressure nearly 1700 times the bulk of the water contained in it. The Boulton and Watt condensing engine took the full advantage of this augmentation of volume, which effected a proportionate displacement of piston, and the condensation of the steam obviated all resisting pressure to the piston.

In the course of the last few years our views of the nature of heat had however undergone a complete change; and, according to the new "dynamic theory," heat, as well as electricity, light, sound, and chemical action, are regarded as different manifestations of motion between the intimate particles of matter, and can be expressed in equivalent values of palpable motion and dynamic effect. In support of this theory, he (Mr. Siemens) could not do better than refer to the able discourses, recently delivered in the Royal Institution, by Mr. Grove and Professor Thomson.*

Viewed from the position of the new theory, the heat given out in the condenser of a steam-engine, represented a loss of mechanical

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* See pp. 152 and 199.

effect, amounting to $\frac{13}{14}$ part of the total heat imparted to the boiler; and the remaining $\frac{1}{14}$ part was all the heat really converted into mechanical effect. The greater proportion of the lost heat might be utilized by a perfect dynamic engine. A vast field for practical discovery was thus opened out; but it might yet be asked whether it was worth while to leave our present tried and approved forms of engines, to seek for economy, however great, in a new direction, considering the vast extent of our coal fields. The reply to this objection was, that the coal in its transit from the pit to the furnace acquired a considerable value, which, for this country, might be estimated at £8 per horse-power per annum (taking a consumption of 13½ tons of coal, at an average expenditure of 12 shillings per ton).

Estimating the total force of the stationary and locomotive engines employed in this country at one million nominal horse-power; it followed that the total expenditure for steam coal amounted to eight millions pounds sterling per annum, of which at least two-thirds might be saved. In other countries, where coal is scarce, the importance of economy becomes still more apparent; but it is of the highest importance for marine engines, the coals whereof had to be purchased at transatlantic stations, at a cost of several pounds per ton, to which must still be added the indirect cost of its carriage by the steamer itself in place of merchandise.

These observations, Mr. Siemens thought, might justify him in bringing before the Institution an engine, the result of nearly ten years' experimental researches, which he thought to be the first practical application of the dynamic theory of heat, of which he was proud to call himself an early disciple. Others, more able than himself, might probably have arrived sooner at a practically useful result; but he might claim for himself at least that strong conviction, approaching enthusiasm, which alone could have given him strength to combat successfully the general discouragement and the serious disappointments he had met with.

The following illustrations, proving the imperishable nature of physical forces and their mutual convertibility, were made use of to indicate more clearly the principles his engine was based upon.

A weight falling over a pulley, to which it was attached by a string, would impart rotary motion to a fly wheel, fixed upon the same axis with the pulley, and the velocity imparted to the wheel would cause the string to wind itself upon the pulley, till the weight had reached nearly its original elevation. If the friction of the spindle and the resistance of the atmosphere could be dispensed with, the weight would be lifted to precisely the same point from whence it fell, before the motion of the wheel was arrested. In descending again, it would impart motion to the wheel as before, and this operation of the weight, of alternately falling and rising, could continue *ad infinitum.* If the string were cut at the instant when the weight

had descended, the rotation of the wheel would continue uniformly, but it might soon be brought to a stop by immersing it in a basin filled with water. In this case the water was the recipient of the force due to the falling weight residing in the wheel; and by repeating the same experiment a sufficient number of times, we should find an increase of temperature in the water, a fact discovered by Joule, in 1843, which first proved the identity of heat and dynamic effect, and established their numerical relation. If the weight falling over a pulley were one pound, and the distance through which it fell one foot, then each impulse given to the wheel would represent one foot pound, or commonly adopted unit of force; and if the water contained in the basin weighed also one pound, it would require 770 repetitions of the experiment of arresting the wheel in the water, before the temperature of that water was increased by one degree Fahrenheit.

Another illustration made use of, was that of a hammer falling *in vacuo* upon a perfectly elastic anvil. The hammer would, under these circumstances, rebound to precisely its original elevation; and granting the perfect elasticity of both hammer and anvil, neither sound nor heat would be produced at the point of concussion. If a piece of copper were suddenly introduced between anvil and hammer, the latter would not rebound, but would make the copper the recipient of the expended force. If the hammer were now lifted again and again by an engine, and the piece of copper were turned about on the anvil, so that at the end of the operation it had precisely the same form as at the commencement, then no outward effect would be produced by the force expended, but the piece of copper would be heated perhaps to redness; and if the engine employed to lift the hammer were perfect, then the heat produced within the copper should be sufficient to sustain its motion.

A familiar instrument for converting force into heat was the fire-syringe. The force expended in compressing the air imparted a sufficient temperature to the same to ignite a piece of German tinder (about 600° Fah.). When the plunger of the syringe was drawn back, it might be observed that the temperature of the enclosed air was again reduced to its original degree, because the heat developed in compression of the air had been spent again in its expansion behind the piston. If the expansion of the heated and compressed air had been without resistance, no reduction of its temperature could have taken place, because no force would be obtained; a fact which had been recently proved by Regnault, and which was perhaps the strongest proof in favour of the dynamic theory of heat that could be brought forward. If the heated and compressed air in the fire-syringe could be produced by some external cause and be introduced behind the plunger after it had descended freely to the bottom, then the force imparted to the plunger in the expansion might be turned to some useful purpose, and a dynamically perfect engine might be obtained. But although the elevated tem-

perature might be readily supplied by means of a fire, it would not be possible to give a sufficient density to the air, except by an expenditure of force in its compression. If, however, heat were applied to a drop of water confined below the plunger till its temperature was raised sufficiently to effect its conversion into steam of the density of the water itself, (Gaignard de la Tour's state of vapours,) and then allowed to expand below the plunger till its temperature was reduced to zero, a dynamically perfect engine would be obtained. The impracticable nature of such an engine was however manifest, if it was considered that steam of the density of the water producing it, would exert a pressure of probably several hundred atmospheres, which pressure the moving part of the engine must be made strong enough to bear at a temperature of more than 1000° Fah., and that the capacity of the working cylinder must be sufficient to allow of an expansion of the steam to several thousand times its original volume. It was therefore necessary to look for other means of obtaining from heat its equivalent value of force, which means, it was contended, were furnished by the "regenerative steam-engine."

This engine, of which several diagrams and a model were exhibited, consisted of three essential parts, namely, the furnace; the working cylinder, with its respirator and heating vessel; and the regenerative cylinder. It consisted also of a boiler and condenser, (unless the steam were discharged into the atmosphere,) but these were not essential to the working of the engine, although of great practical utility. The regenerative cylinder had for its object alternately to charge and discharge two working cylinders, and the action of its piston might be compared to that of a hammer oscillating between two elastic anvils. The regenerative cylinder communicated at its one extremity with one working cylinder, and at the other extremity with another and similar working cylinder, and these communications were not intercepted by valves. The working cylinders were so constituted that their capacity for steam of constant pressure was the same, no matter where the working piston stood. Each consisted of a cylinder of cast iron, open at both ends, which was completely enclosed in another cylinder or heating vessel, one end of which was exposed to the action of a fire. Within the inner cylinder was a large hollow piston, filled with non-conducting material, to which was attached a long trunk or enlarged hollow piston rod of nearly half the sectional area of the piston itself. This trunk was attached to the working crank of the engine in the usual manner. The trunk of the second working cylinder stood precisely opposite, and was connected with the same crank. The piston of the regenerative cylinder was also connected with the same crank, but stood at right angles to the two working cylinders. The consequence of this arrangement was, that while the two working trunks made their strokes (the one inward and the other outward) the piston of the regenerative cylinder remained comparatively quiescent upon its turning or dead point, and *vice versâ*. Around the two

heating vessels boilers were disposed, which received the heat of the fire, after it had acted upon the former. The steam generated within the boilers was introduced into the engine by means of an ordinary slide valve (of comparatively very small dimensions) at short intervals, and when the piston of the regenerative cylinder was in its extreme position. The admission of the steam, which was of high pressure, took place on that side of the regenerative cylinder where compression by the motion of its piston had already taken place, and at the same instant a corresponding escape of expanded steam on the other side of the regenerative piston was allowed to take place into the atmosphere. The quantity of steam freshly admitted at each stroke did, however, not exceed one-tenth part of the steam contained in the working cylinders of the engine, and served to renew the same by degrees, while it added its own expansive force to the effect of the engine. The compression of the steam into either of the working cylinders took place when its hollow piston stood at the bottom. While in this position the steam occupied the annular chamber between the working trunk and the cylinder, besides the narrow space between the cylinder and the surrounding heating vessel. The pressure of the steam being the same above and below the hollow piston, but the effective area below being equal to twice the area above, the working trunk, attached to the piston, would be forced outward through the stuffing box, while the steam of the annular chamber above the piston passed through the narrow space intervening, into a space of twice the capacity of the annular chamber below the hollow piston. During its passage the steam had to traverse a mass of metallic wire gauze or plates, (the respirator, presenting a large aggregate surface,) which reached at one end sufficiently downward into the heating vessel so that its temperature was raised from 600 to 700° Fah., while its other extremity remained at the temperature of saturated steam, or about 250° Fah. In consequence of the addition of temperature the steam received on its passage through the respirator, its elastic force was doubled, and it therefore filled the larger capacity below the hollow piston or displacer without loss of pressure. When the effective stroke of the working trunk was nearly completed, the regenerative piston commenced to recede, and the steam below the hollow piston expanded into the regenerative cylinder, depositing on its regress through the respirator the heat it had received on its egress through the same, less only the quantity that had been lost in its expansion below the working piston, and which was converted into dynamic effect or engine power, and which had to be supplied by the fire. The expansion and simultaneous reduction of temperature of the steam caused a diminution of its pressure from four to nearly one atmosphere; and the working trunk could now effect its return stroke without opposing pressure, and while the second working trunk made its effective or outward stroke impelled by a *pressure of four* atmospheres.

The respirator, which was invented by the Rev. Mr. Stirling, of Dundee, in 1816, fulfilled its office with surprising rapidity and perfection, if it were made of suitable proportions. Its action was proved at the end of the discourse by a working model. It had been applied without success to hot-air engines by Stirling and Ericsson, but failed for want of proper application; for it had been assumed (in accordance with the material theory of heat) that it was capable of recovering all heat imparted to the air, and, in consequence, no sufficient provision of heating apparatus had been made. It having been found impossible to produce, what in effect would have been a perpetual motion, the respirator had been discarded entirely, and was even now looked upon with great suspicion by engineers and men of science. Mr. Siemens had, however, no doubt that its real merits to recover heat that could not practically be converted by one single operation into mechanical effect, would be better appreciated. The rapidity with which the temperature of a volume of steam was raised from 250° to 650° Fah. by means of a respirator, was indicated by the fact that he had obtained with his engines a velocity of 150 revolutions per minute. The single action of heating the steam occupied only a quarter the time of the entire revolution of the engine, and it followed that it was accomplished in one-tenth part of a second. But, in explanation of this phenomenon, it was contended, that the transmitting of a given amount of heat from a hotter to a cooler body, was proportionate to the heating surface multiplied by the time occupied, and that the latter factor might be reduced *ad libitum*, by increasing the former proportionately. The air-engines of Stirling and Ericsson had failed also, because their heated cylinders had been rapidly destroyed by the fire; but the cause for this was, that an insufficient extent of heating surface had been provided, and it was well known that even a steam-boiler would be rapidly destroyed under such circumstances. Mr. Siemens was led by his own experience to believe that his heating vessels would last certainly from three to five years; and being only a piece of rough casting, that could be replaced in a few hours, and at a cost below that of a slight boiler repair, he considered that he had practically solved the difficulty arising from high temperature. It was however important to add, that all the working parts of his engine were at the temperature of saturated steam, and therefore in the same condition of ordinary steam-engines; whereas in Ericsson's engine, the hot air had entered the working cylinder. In surrounding the heating vessel with the boiler, an excessive accumulation of heat was prevented from taking place, and the pressure of the steam in the boiler became the true index to the engine-driver of the temperature of the heating vessel. Another essential property of the heating vessel was, that all its parts should be free to expand by heat without straining other parts, which was accomplished by a free suspension, and by undulating its surface. Lastly, it should be massive, to withstand the fire with impunity, for iron

was, strictly speaking, a combustible material. The pyropherus or finely divided metallic iron, took fire spontaneously on exposure to the atmosphere, a chip of iron was ignited in flying through the flame of a candle; an iron tea-kettle was destroyed by exposing it (unfilled with water) to a kitchen fire; whereas, in forging a crank shaft, the solid mass of iron withstands the white heat of the forge fire for several weeks without deteriorating. A heating vessel, properly constructed and protected, might be heated with safety to 700° Fah., at which temperature it would be almost as able to resist pressure, as at the ordinary temperature of the atmosphere, the point of maximum strength of iron being at 550° Fah., as had been proved by experiments made for the Franklin Institute. The construction of a heating vessel combining these desiderata was of paramount importance for the success of Mr. Siemens' engine, and had not been accomplished without combating against considerable practical difficulty.

Although heat may be entirely converted into mechanical effect, it would nevertheless be impossible to construct an engine capable of fulfilling this condition without causing at the same time a portion of heat to be transferred from a hotter to a cooler body, and which must ultimately be discharged. This necessity has been generally proved, and in a very elegant manner, by Professor Clausius, of Zürich, and implies at least the partial truth of "Carnot's theory." In the "regenerative steam-engine," provision had been made for absorbing this quantity of heat, arising in this case from the circumstance, that the saturated steam enters the respirator in a state of greatest density or compression, and returns through it (expanding into the regenerative cylinder) at a gradually diminishing density, although the temperature of the extreme edges of the respirator remains proportionate to the condensing point of the steam of greatest density, by providing water chambers about the cover of the working cylinder, and around the regenerative cylinder, which are in communication with the steam-boiler. The heat absorbed from the slightly superheated steam is thus rendered useful to generate fresh steam.

Objection had been raised by casual observers against the regenerative steam-engine, on account of its apparent similarity in principle to the "air-engines" of Stirling and Ericsson, implying similar sources of failure. The apparent similarity in principle arose from the circumstance that both Stirling and Ericsson, as well as himself, had employed the respirator and high temperatures; but these were but subordinate means or appliances, that might be resorted to in carrying out a correct as well as an erroneous principle.

In the winter of 1852-53, when Ericsson was engaged upon his gigantic experiment in America, the speaker had had occasion to read a paper to the Institution of Civil Engineers, entitled, "On the conversion of heat into mechanical effect," wherein he had endeavoured to set forth the causes of the probable failure of that

experiment, and to guard against a sweeping condemnation on that account of some of the means Ericsson had employed.

According to the dynamic theory of heat, the elastic medium employed in a perfect caloric engine was a matter of indifference, and air had been resorted to, because it was perfectly elastic, and always at hand. In practice, however, the elastic medium employed was a matter of very great importance, and he (Mr. Siemens) had given the decided preference to steam, and for the following reasons :—

1. The co-efficient of expansion of saturated steam by heat exceeded that of air in the proportion of about 3 : 2, but decreased with an increase of temperature. This was not in accordance with the established rule by Gay-Lussac and Dalton, but was the result of his own experiments (described in a paper, "On the expansion of steam, and the total heat of steam," communicated to the Institution of Mechanical Engineers, in 1850), and had been borne out by his practical experience on a large scale. Mr. Siemens had been first induced to undertake these experiments in consequence of an observation by Faraday, that the elastic force of the more permanent vapours gave way rapidly, when by abstraction of heat their points of condensation was nearly obtained. He conceived that gases and vapours would expand equally by heat, when compared, not indeed at the same temperature, but at temperatures equally removed from their points of condensation.

2. When saturated steam was compressed (within the regenerative cylinder), its temperature would not rise considerably (as the fire-syringe evinced in respect of air), because Regnault had proved that the total heat of steam increased with its density, and consequently the heat generated in compression was required by the denser steam to prevent its actual condensation. Without this fortunate circumstance, the steam would be heated already by compression to such an extent, that it would be difficult indeed to double its elastic force by the further addition of heat in the respirator.

3. Steam exercised no chemical action upon the metal of the heating vessel and respirator, because the oxygen it contained was engaged by hydrogen, which latter had the stronger affinity for it until a white heat was reached; whereas the free oxygen of atmospheric air attacked iron and brass at much lower temperatures.

4. The specific gravity of steam was only about one-half that of atmospheric air at equal temperature and pressure; moreover it was a far better conductor of heat, and both circumstances qualified it for rapid respirative action.

5. The fresh steam required for starting and sustaining the power of the engine was generated by heat that would otherwise be lost. No air-pumps, &c., were required, and the management of the engine became as simple as that of an ordinary high-pressure steam-engine.

Varying Cubic Space.

	Cubic Feet.		Cubic Feet.
In a slave ship, with 311 persons	14	Hospitals, Dundee (old, now destroyed)	398
Best slave ships	28	" Liverpool	561
Emigrant ships, upper deck	90	" Glasgow	750
" lower do. 7ft. high	126	" Walton (convalescent)	800
" " if under	173	" Middlesex	1000
H.M.S. Rodney (sleeping space)	76	" Edinburgh	1090
" Ariel	94	" Haslar	1100
" Ajax	98	" Westminster	1200
" Falcon	104	" Guy's (old wards)	1200
" Severn	117	" Newcastle	1500
" Pylades	125	" Dundee (new)	1545
" Duke of Wellington	128	" King's College	1600
" Impérieuse	145½	" St. Bartholomew's	1650
		" Guy's (new wards)	1700
		" London	1700

As a striking example of the error which prevails regarding the cubic space necessary for health, and as a good instance of the worthlessness of the appeal to practical experience, in many similar cases, I may give the following police regulation for lodging-houses :—

"The space allowed in common lodging-houses for each lodger, in rooms from 5 ft. 6 in. to 6 ft. in height, is 50 superficial feet; and in rooms more than 6 ft. in height, 30 superficial feet are allowed for each lodger.

"This arrangement has been found to work satisfactorily, and to secure the health of the lodgers. Two children under 10 years of age are reckoned as one adult."

Police Allowance in Lodging-Houses.

When 5 ft. 6 in. to 6 ft. high,	50 superficial feet	=	275 to 300 cubic feet.
When 6 ft. 1 in. high,	30 "	=	183 cubic feet.

That is, rooms from 5 ft. 6 in. to 6 ft. high give from 275 to 300 cubic feet for each person; and if 6 ft. 1 in. high, then only 183 cubic feet are given.

To obtain an equal amount of cubic feet of air the rooms should be between 9 and 10 ft. high. The police rule is, however, justified by experience! "This arrangement has been found to work satisfactorily, and to secure the health of the lodgers." This does not prove the truth of the rule, but only that there is some great mistake in the rule.

What rule then must be made? It appears to me that instead of taking the cubic contents of a room as the guide, the ventilation and the square contents, or in other words, the change of air and the size of the floor, can alone determine the number of persons *that can safely* and properly be admitted into any space.

As then we have learnt that a fixed amount of cubic space will not give us what air we want, or tell us what we have, we will turn to the question, How much air we do want, and how are we to know when we have got it? According to the best experiments on

RESPIRATION,

Man inhales 15,885 cubic inches of oxygen in 12 hours.
44 cubic feet of atmospheric air contain this oxygen.
3·7 per hour, = 0·06 cubic foot per minute.

Man expires . . 160 cubic feet in 12 hours, containing 4 per cent carbonic acid
If fresh air . . 160 „ are added, there will be 2 per cent.
If then fresh air . 320 „ are added, there will be 1 per cent.
Hence, if a man has 640 „ in 12 hours, it will contain 1 per cent.
= 53·3 „ per hour, = 0·88 per minute.
= rather less than 1 cubic foot per minute.

In other words, for diluting the carbonic acid we require 14½ times more air than for the supply of oxygen.

In different ages, sexes, states, and conditions, the variation in these numbers is great, and no experiments have yet been made with human beings in the best and most natural conditions.* The following variations, obtained by single inspirations, may show how easily errors may be made—

EFFECT OF RAPID BREATHING ON THE AMOUNT OF EXPIRED CARBONIC ACID.

Number of expirations in a minute.	Duration of each in seconds.	Carbonic acid in 100 vols. expired air.	Constant amount of Carbonic acid.	Proportional increase.
192	0·3125	2·6	2·6	0·
96	0·625	2·7	2·6	0·1
48	1·25	2·9	2·6	0·3
24	2·5	3·3	2·6	0·7
12	5·0	4·1	2·6	1·5
6	10·0	5·7	2·6	3·1

EFFECT OF HOLDING THE BREATH ON THE AMOUNT OF CARBONIC ACID EXPIRED.

Ordinary Respiration.		*Extraordinary deep Respiration.*	
Breath held.	Carbonic acid in expired air.	Breath held.	Carbonic acid in expired air.
20 seconds.	6·03 per 100 vols.	20 seconds.	4·80 per 100 vols.
25 „	6·18 „	40 „	5·21 „
30 „	6·39 „	60 „	6·06 „
40 „	6·62 „	80 „	6·44 „
50 „	6·62 „	90 „	6·50 „
60 „	6·72 „	100 „	8·06 „

* Even the experiments of MM. Regnault and Reiset on the respiration of animals, though perfect in their physical and chemical arrangements, are far from conclusive, in consequence of the physiological conditions to which the animals were subjected.

Moreover, we have no accurate experiments to show the smallest quantity of carbonic acid that is injurious in the longest time. Is one per cent. of carbonic acid in air breathed for 12 hours injurious? Is half a per cent. injurious in 24 hours? Is a quarter per cent. hurtful if breathed for 48 hours or longer? Assuming that about one cubic foot of fresh air per minute gives to a man about one per cent. of carbonic acid in air he breathes—

2 cubic feet	would give	. $\frac{1}{2}$ per cent.
(2$\frac{1}{2}$	,,	minimum, according to Vierort)	.
3 to 4	,,	Dr. Arnott considers insufficient	. $\frac{1}{3}$ to $\frac{1}{4}$,,
10	,,	Dr. Reed advises	. $\frac{1}{10}$,,
20	,,	Dr. Arnott advises	. $\frac{1}{20}$,,

Is then one part of carbonic acid in 2000 parts of air injurious when continually breathed? The atmosphere contains 3 parts carbonic acid in 10,000 parts of air when most pure, that is 1 part carbonic acid in 3333 parts of air.

Hence Drs. Arnott and Reed have given the highest limit, whilst the lowest healthy limit is what we require to enable us to decide when we have got as much air as we want.

From the best experiments, namely, those of M. Leblanc, I shall assume that air containing one per cent. of carbonic acid indicates such an impure state of atmosphere, that if breathed for 12 hours there will be an injurious action on the system; and that air containing half a per cent. of carbonic acid breathed continuously for 24 hours or more will probably prove hurtful. Whether atmospheres having this amount of impurity are more injurious to women and children, though probable, is also unproved. Whether this amount of impurity, acting day after day, becomes more tolerated by the system is very doubtful. On all these questions much might be said, and much has yet to be done. But the question at present to be answered is, How are we to know when the air is thus far impure? What means do we possess of determining the amount of ventilation in this or any other room?

There are three methods of obtaining this knowledge. The physiological method; the physical method; and the chemical method.

1. The physiological method consists in the determination of the action of the air of any room on its inmates, when well or when ill. The offensiveness of a room to a person just entering it, that is the action of the air on the sense of smell, is liable to great variations, depending on the observer himself. The nerves do not measure actual amounts of impression, but only variations arising from different degrees of impression; and the action on the nervous system of one person is not a measure of the impression on the nerves of another person. Moreover, it is far from proved that the offensive smelling substances are poisonous. They may be more

poisonous than carbonic acid, but we do not know that this is a fact. The best example I can give you of an animal substance in a state of decomposition is musk, and yet it is no highly poisonous substance; but even if these animal substances are not poisonous, still, as the offensiveness perceived by a person just entering from the fresh air bears an inverse proportion to the dilution of the air in the room by fresh air, the offensiveness to such a person may be taken as one test of the want of ventilation.

Another part of the physiological method consists in determining the general action of the air of the room on the body inside as well as outside, on the nerves, muscles, skin, and mucous membranes.

Carbonic acid, in very large doses, immediately destroys life by stopping the breath, but of this rapid action there is no question. What we want to know is,—What is the action of the smallest injurious doses? Carbonic acid, like other poisons in small doses, is a medicine. We take it in soda water, and in effervescing medicines to allay irritation. It is used as a douche; at first exciting the eyes or the nose, and soon allaying irritation.

Like other medicines its effects vary with the age of the person taking it, with the time during which it is continued, with the strength of the dose, and with the peculiarities of the individual. A very small dose, long continued, will produce effects, whilst the same dose, in a short time, may give no perceptible result. The action of a full overdose may be well compared with the action of æther, of an overdose of alcohol, or of chloroform. At first irritation, then slight giddiness, intense giddiness, desire to vomit, excessive prostration, inability to make any muscular effort, syncope, death.

The symptoms from small doses, long continued, closely resemble the symptoms from smaller doses of alcohol, long continued; slowly the nutrition of the textures of the body is affected; debility, unhealthy blood, passive congestions, low inflammations, especially of the mucous membranes, and broken skin, with ulceration and gangrene, are produced.

If these effects are observed in any atmosphere, then the ventilation is proved to be insufficient.

2. The physical method consists in determining the velocity of the air passing out of the room or into the room, either by calculation or by experiment.

The mean temperature of the air in the chimney is determined. Hence the increase in volume of the air is known. This volume of heated air is then compared with the volume of an equal weight of cold air. The difference in height in these two volumes of air is obtained, and the force of the draught is equal to the velocity which a heavy body would acquire by falling freely through this height. The velocity of a falling body, in feet per second, is equal

to eight times the square root of the number of feet in the fall. Hence the velocity of the air in the chimney per second is eight times the square root of the difference in the height of the two volumes of air. From this, the friction of the air in the chimney must be deducted; this varies directly as the length, and as the square of the velocity, and inversely as the diameter; usually from one-third to one-fourth, must be deducted, and then multiplying by 60 the velocity of the air per minute is found; and multiplying the velocity per minute by the area of the chimney, the number of cubic feet of air discharged per minute is known; that is, when the entrance of air is as free as the exit.

Another method consists in determining the rate of motion of the air per minute by an anemometer, by multiplying the area of the narrower part of the chimney by the velocity, the number of cubic feet of air per minute passing out of the room may be obtained.

The determination of the rate of motion of the air passing into the room is still more difficult; no two openings into the room give the same velocity, even if they are the same size, unless the temperature of the air on the sides of the two openings is exactly the same.

Moreover, in all the physical methods the temperature of the external air is constantly changing; and the heat of the air in the chimney is liable to great variations; and the occasional ventilation caused by opening the doors and windows interferes with the accurate determination of the amount of constant ventilation.

Though the physical method alone is still very imperfect, yet, with the physiological method, it constitutes almost all the evidence that has hitherto been sought in doubtful cases.

3. The chemical method consists in weighing or measuring the products of combustion in the room. These products are heat, water, and carbonic acid; possibly small quantites of other substances are produced, but they cannot be determined quantitatively. Moreover, the animal heat is so easily lost, and other sources of variations in the temperature so interfere with the measurement of the heat produced in the body, that it can afford no help.

The amount of moisture in the room and in the internal air may be found by experiment; and assuming that each adult man by respiration produces 3857 grs. of water in 24 hours, the quantity of moisture which would be present if the room were closed may be determined by calculation; hence the quantity of air which has escaped may be known. The same method may be followed with the carbonic acid, which is a poison, and though it exists in the atmosphere still it is only in very small and nearly constant amount.

Hence the chemical method mainly consists in determining how much carbonic acid exists in any space in a given time, when a

given number of people have remained in it. Then the quantity of carbonic acid which this number of people would produce in the given time must be calculated, and by deducting the quantity found, from the total quantity produced, the quantity of air which escaped from the given space in that time can be determined.

The following examples from M. Leblanc's paper will best illustrate this method.

M. Leblanc remained himself for ten hours in a perfectly closed atmosphere, the capacity of the chamber = 459 cubic feet (13 met. cube); this gave him less than one cubic foot of air per minute (= 0·76 cubic foot). At the end of this time he found the carbonic acid = 0.0075 in volume, or one part carbonic acid in one hundred and thirty-three parts of air.

He found in a soldier's sleeping-room 25 men in a cubic space, which if perfectly closed would have given about 0·8 cubic foot per minute per man. Analysis gave 3 parts carbonic acid in 1000 air. Had the room been perfectly closed whilst they slept, there should have been 9 parts carbonic acid. Hence, 2·4 cubic feet of air per minute had been given.

In another sleeping room, with 52 soldiers, the capacity of the room would have given 0·6 cubic foot per minute, per man, 3 parts carbonic acid per 1000 were found; if perfectly closed, there would have been 10 parts. Hence, about 2 cubic feet per minute had been given to each man.

In a much smaller and worse ventilated room, with 11 soldiers and of capacity about 0·5 cubic foot per minute per man, nearly 9 parts of carbonic acid were found in 1000 air. If closed, there would have been 14 parts. Hence only 0·8 cubic foot of air per minute was given to each man.

The method followed by M. Leblanc for determining the carbonic acid was the following:—an aspirator which held about 43 pints of water had tubes fitted to it, for absorbing water and carbonic acid. In the course of one hour about 1456 cubic inches, or ·844 cubic foot of air was drawn through the tubes by the escape of the water; on weighing the tube which absorbed the carbonic acid in the second experiment mentioned above, there was a gain of 2·62 grs. corresponding to 5·2 cubic inches of carbonic acid.

In his previous experiments on confined air, M. Leblanc used two large globes, holding each about 32 pints, which were exhausted and then filled with the air to be examined; this was drawn through the absorbing tubes by two other exhausted globes.

For my experiments on the close air in St. Pancras workhouse, I obtained a long tube, which, through the kindness of Mr. De la Rue, was accurately graduated; this I filled myself with air in the rooms; the tube was then closed and brought to the laboratory of the College of Chemistry, placed over mercury, and potass bulbs were introduced by Mr. Witt, and the height of the mercury noted. After 12 hours, the height was again noted, and by corrections for

the temperature and pressure, the absorption of the carbonic acid was determined. Thus,

In one room there was	1·14	per cent. in volume of carbonic acid.		
In another	„	2·75 and 2·02	„	„
In another	„	1·8	„	„
In another	„	1·6	„	„

The absorption is so small, and the corrections so considerable, that it would be far more desirable, instead of measuring the result, to determine the carbonic acid by weight, if a convenient apparatus could be constructed. Through the kindness of Mr. Defries, I am enabled to show you a gas meter, which, by the action of a falling weight, draws the air through an absorbing apparatus, and registers the amount of air which has passed through. By weighing the absorbing apparatus before and after the passage of the air, the amount of carbonic acid may be determined; and by noting the index of the register, the amount of air in which this carbonic acid was present can be known.

If another gas meter were placed before the absorbing apparatus; that is, if an absorbing apparatus were placed between two gas meters, when the air was passed through both meters, the difference in the two registers would give the measure of the amount of carbonic acid, whilst the weight of the carbonic acid might also be determined by weighing the absorbing apparatus.

It must be remembered that carbonic acid may not be the sole poison in expired air; and an accurate investigation is yet wanting to show what other substances are injurious, and how they may be best determined quantitatively. When this is done, but not till then, as in our supply of water, so in our supply of air, we shall cease to trust to a physiological or physical method of enquiring what we want or what we have got, but we shall rely on chemistry to determine the purity of the air we breathe, just as much as we now trust to it for determining the quality of the water which we drink.

I might in conclusion point to this statement, given to me by a physician to a lying-in-hospital—

Mortality of Mothers.	Deaths.
During 4½ years, before systematic ventilation	60
During 7 years, with Dr. Reed's system of ventilation	9
During 4 years again, without it	24

Mortality of Children.	
During 5½ years, with the ventilation	6
During 4 years, without it	36

But some other causes might be thought to produce this result. I will therefore pass on to the question—

Why do we want ventilation? Why do we want fresh air?

Why do we want to take in oxygen? and why do we want to get rid of carbonic acid?

Shortly, we want oxygen, because of its chemical energy. It is the main spring of our life. On it the production of animal heat depends, and the vital powers—sensation and motion, no less than nutrition and secretion, are directly influenced by its action.

Why do we want to get rid of surrounding carbonic acid? Literally, because the carbonic acid stops the way, and prevents the escape of newly formed carbonic acid from within. If we were placed in an atmosphere containing as much carbonic acid as exists in the lungs, the carbonic acid of the atmosphere would not pass from the lungs to the blood and act as a poison, but that carbonic acid which was passing out from the blood would stop in the lungs and prevent more from escaping out of the blood, and that carbonic acid which was formed in the body would act as a narcotic poison. From experiments on animals it appears, that the air must contain 20 per cent. of carbonic acid before absorption of that gas by the blood is observed. Moreover the escape of gases from the blood affects the circulation of the blood. In sudden death from suffocation, the side of the heart which throws the blood to the lungs is found distended, whilst the side which throws the blood from the heart is empty, there has been an obstruction to the flow. By stopping respiration and causing pressure we can stop the pulse and the heart's sounds and impulse when we please. This experiment is easy to make. There can be no doubt that this is more the result of pressure than of any arrest of escape of carbonic acid. I mention it only as a striking evidence how suddenly the action of the heart may be influenced by the respiration. When the escape of carbonic acid from the blood is retarded or prevented, the want of ventilation of the blood causes more or less stoppage of the blood in the vessels, and makes the blood a narcotic poison to all the tissues with which it is in contact.

We may consider oxygen as our most necessary food, and carbonic acid as the refuse which passes into our sewers. We have all probably come to the full belief that a house badly drained causes disease and death; but we hardly yet fully admit to ourselves that a house or body without good means of ventilation is a house or body badly drained. At present our chimneys are our chief aerial drains, which almost cease to act as soon as the temperature outside and inside the house is the same; and even when these drains are in action, we are unwilling to think that that fire which so cheerfully ministers to our warmth, like most human contrivances for doing two things at once, does neither well.

[H. B. J.]

EXTRA EVENING MEETING,

Monday, April 21.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Humphry Sandwith, M.D.
CHIEF OF THE MEDICAL STAFF AT KARS.

On the Siege of Kars.

On the breaking out of the war in 1854, Dr. Sandwith, residing at Constantinople, left that city for the Danube, and went through a campaign. In the autumn of the same year he was appointed to the staff of General Williams, and shortly after joined him at Erzeroom. In June, 1855, he accompanied him to Kars. They found the defences of the city much strengthened by the exertions of Colonel Lake, aided by Captains Thompson and Teesdale. On June 18, General Mouravieff, with an army of 40,000 infantry and 10,000 cavalry, came in sight of the city. The army of the besieged consisted of 15,000 men, with only three months' food, and three days' ammunition; nevertheless in a short time, by the energy, skill, firmness, and kindness of General Williams and his staff, the enthusiasm of the defenders of Kars was so thoroughly roused as to enable them to endure, with patient heroism, the sufferings of this protracted siege, so greatly aggravated by disease and deficient resources.

The first skirmish with the enemy took place on June 14. On July 15, Kars was thoroughly blockaded; and the Russian camp drew nearer and nearer. Mouravieff reconnoitred Erzeroom, but returned to Kars early in August. In his absence the Russians attacked the place, but were repulsed with severe loss (August 7). On the 8th of September the sufferings of the besieged were greatly relieved by the discovery of a quantity of secreted corn; but on the 25th the cholera broke out.

The grand assault on the place by the Russian army took place just before break of day, on the 29th of September. The western extremity of the works named Tahmasp was attacked simultaneously with the line of forts in the rear of the town called the English batteries. These last being entrusted chiefly to irregular troops were carried, and the Russians commenced shelling the town from this position. In about three hours, however, the enemy being

attacked in both flanks, and menaced in front, was obliged to retire, having suffered severely; but he still persisted in his attacks on Tahmasp, which continued almost without intermission for seven hours. This position was defended by General Kmety and Major Teesdale; and the troops were mainly riflemen, armed with the *carabin à tige*. About noon the enemy retired, leaving upwards of 6000 dead under the breastworks and batteries of Kars. General Williams, from the centre of the camp, directed each grand movement during the day.

Dr. Sandwith stated his belief, that if General Williams had had 2000 cavalry, the enemy would have been totally defeated and dispersed.

The Russian cavalry, 10,000 strong, having scarcely suffered during the assault, was enabled to keep up the blockade; so that the state of the garrison, suffering from cholera and famine, became hopeless. In the later days about 100 of the troops alone died of famine, while the condition of the townspeople was desperate in the extreme; the carcasses of dead animals were torn from their graves and devoured by the men, women, and children, while the grass in all the open spaces was torn up to be eaten. When human nature could endure no longer, it was resolved to capitulate.

On November 25, General Williams and his aide-de-camp Teesdale, rode over, under a flag of truce, to the Russian camp. They were well received by Mouravieff. General Williams told his chivalrous enemy that he had no wish to rob him of his laurels; the fortress contained a large train of artillery, with numerous standards, and a variety of arms: but the army had not yet surrendered, nor would it without certain articles of capitulation.

"If you grant not these," added the General, "every gun shall be burst, every standard burnt, every trophy destroyed, and you may then work your will on a famished crowd." "I have no wish," answered Mouravieff, "to wreak an unworthy vengeance on a gallant and long-suffering army, which has covered itself with glory, and only yields to famine. Look here," he exclaimed, pointing to a lump of bread, and a handful of roots, "what splendid troops must these be, who can stand to their arms in this severe climate on food such as this! General Williams, you have made yourself a name in history; and posterity will stand amazed at the endurance, the courage, and the discipline which this siege has called forth in the remains of an army. Let us arrange a capitulation that will satisfy the demands of war, without outraging humanity."

The terms of capitulation were briefly as follows:—"The officers and soldiers of the regular army were to pile arms in camp, and march out with their music and colours, and surrender themselves prisoners of war to the Russian army, retaining their swords. All private property, the castle, mosques, and other public buildings, are to be respected, and the inhabitants protected from pillage

or insult. The militia, the Bashi-Bozooks, are allowed to depart unarmed to their homes. The medical corps, and other non-combatants, are to be released and be free to serve again in any other army. A certain number of foreign officers, and the subjects of states not at war with Russia, are to be allowed to depart, on condition of not serving again during the continuance of the war." On the 27th, General Williams and his staff dined with the Russian general. Even in their desperate circumstances, the capitulation was received by the army with great lamentation.

Dr. Sandwith left Kars on November 30, and arrived in London on the 9th of January, 1856.

[H. S.]

WEEKLY EVENING MEETING,

Friday, April 25.

Sir Benjamin Collins Brodie, Bart. D.C.L. F.R.S. Vice-President, in the Chair.

W. B. Donne, Esq.

On the Works of Chaucer, considered as Historical Illustrations of England in the 14th Century.

Mr. Donne commenced his discourse with some remarks on the changes which the language and literature of England had undergone since the age of Edward III.

In the period of time which elapsed between Chaucer's birth and death, 1328—1400, occurred some of the most striking and stirring events in English history. It includes the reigns of Edward III. and Richard II., and the opening scenes of the usurpation of the House of Lancaster. It comprises Edward's French and Scottish wars—his league with Jaques van Artevelde and the men of Ghent—so important at the time and in its consequences to English commerce and manufactures,—the battle of Crecy, where the Black Prince won his spurs, the battle of Poictiers, where he approved himself a soldier fit to stand by Cæsar and give direction. Chaucer may possibly have been among the crowd that welcomed that prince when he rode beside John of France through the streets of London to the palace of Westminster; he may have been among the spectators of the banquet at which Edward entertained the three captive majesties of France, Scotland, and Cyprus.

Perhaps to Chaucer not the least interesting result of these wars was the presence of those Provençal minstrels, who came in the train of the Black Prince. Nor was the reign of Richard II. unpropitious to men of letters. The court was gay, but refined; the monarch, though unwise, was not unlettered. He patronised Gower, and was not unmindful of Gower's pupil and friend. There was a rising, too, of the commonalty in this reign, not unmarked by Chaucer, who employs it as an image of rural confusion. There was banding of the country-party against the courtiers: schisms in the country-party itself. Harry of Lancaster's banishment and return, afterwards chronicled by Shakspeare in scenes

> "Sad, high, and working full of state and woe
> Such noble scenes as draw the eye to flow."

A king discrowned and swiftly or lingeringly murdered, and the seeds sown of the great barons' war, which, before another half century had passed away, convulsed England from the Exe to the Tweed. It was a change not much noted at the time, yet fraught with consequences more durable than the humiliation of France, that in Edward's reign the laws began to speak in the English tongue, and the power of the minor barons, afterwards the Commons of England, to be felt in Parliament. It was a movement much noticed at the time, yet without perception of its full results, that Wickliffe was not merely permitted to assail the doctrine and discipline of the church, but was also encouraged in his assault by the first prince of the blood, and by some of the foremost men in the realm.

Of this period Chaucer was for at least sixty years an attentive observer, and latterly an accurate chronicler. In its movements he took part to a degree unusual with poets: and when not taking part was taking notes of this brave, bustling, and youthful people of England. And his opportunities for observation were most favourable. Were we to seek for a capable historian of an age we should be inclined to repeat Agur's prayer—"Give him neither poverty nor riches." Too highly placed, the observer is captived by the prejudices of his rank: too lowly, his field of contemplation is narrowed. Chaucer occupied a middle position. He was connected by birth with the middle order; by marriage with the higher; his employments brought him into contact with the people; his gifts and his learning rendered him an acceptable companion to the most cultivated persons of his age. His occupations, at different periods of his life, were well adapted to his functions as the chronicler of manners. As Commissioner of Customs, he was enabled to study the commercial classes; as Clerk of the Works and Ranger of the Royal Forests, he was familiar with artisans and husbandmen. His military service acquainted him with camps; his diplomatic missions with cabinets. That he was the most observing of all observers is plain from the Prologue to his "Canterbury Tales."

The speaker next observed it was not surprising that the biography of Chaucer was scanty and doubtful. Little is known of Shakspeare and his contemporaries; although, when they flourished printing was common, and the great writers of the age were either, like Raleigh and Sidney, the most distinguished of public men, or like Heywood, Shakspeare, and Jonson, constantly before the public either as actors or authors. Whereas Chaucer, and his contemporaries, wrote sixty years before Caxton set up the first printing-press at Westminster, and there was no theatre to diffuse and perpetuate their fame, and few readers to take an interest in their writings. Enough, however, is ascertained of Chaucer's history to warrant us in describing him as a "courtier, soldier, and scholar."

A courtier.—If Edward III. were not a very zealous patron of literature, he was a favourable and fostering friend to Chaucer himself. Perhaps he owed his promotion, in some degree, to a fortunate marriage. John of Gaunt and Chaucer espoused two sisters, the daughters of Sir Payne Roet, a native of Hainault, and Guienne King-of-arms. The poet thus came under the immediate notice of Queen Philippa of Hainault, and of the Duke of Lancaster. The Duke's regard for his wife's sister was manifested by a pension, by occasional presents, and her husband's advancement. In 1367 Chaucer was made one of the valets of the king's chamber, in 1370 he was employed in the king's service abroad, and towards the end of 1372 he was one of a commission to determine upon an English port where a Genoese commercial establishment might be formed. On this occasion he visited Florence and Genoa. In 1374 he was appointed Comptroller of the Customs in the port of London; and soon afterwards sent, in association with Sir Thomas Percy (afterwards Earl of Worcester), on a special mission to Flanders. In 1386 he sat in Parliament as Knight of the Shire for Kent, and although he latterly met with reverses, and fell with the Lancaster party for a time, yet on the accesion of "high-mounting Bolingbroke" he enjoyed at the last the full sunshine of royal favour. As he does not appear to have been a very zealous soldier, so it is probable that he was not a very active partisan. Though, like the great Florentine, driven by his political enemies into banishment, he expends on them no withering sarcasms, and even his occasional allusions to seasons of adversity are steeped in good humour.

A soldier.—In Chaucer's age the tonsure of the priest was almost the only mode of exemption from the bearing of arms, and both gentleman and churl would have been deemed recreant had they eluded their term of military suit and service. From Chaucer's own testimony, it is known that in the autumn of 1359 he accompanied that gallant and well-appointed army with which Edward III. invaded France. This was apparently the first and last of the poet's campaigns. It was ill adapted to stimulate his military ardour, if, indeed, he possessed any, for the expedition was

nearly equally disastrous to the invaders and the invaded; pestilence and famine paged the heels of the English host, and no crowning victory like Crecy compensated for the loss and discredit of the expedition. Chaucer himself became for a while the inmate of a French prison. He was released at the Peace of Bretigny, or Chartres, in the following year.

From the internal evidence of his writings it is probable that the trade of war was not much to his taste. Neither the Norman-French poets, from whom he borrowed so largely in his earlier writings, nor the romance writers of the 12th and 13th centuries, imparted to him their Homeric fondness for stricken fields and blazing towns. Complying, indeed, with the customs of literature as he found them established, he occasionally describes passages of arms, and the gests and graces of the tournament. But he does not dwell with any zest upon such themes, and forsakes them willingly for more peaceful subjects. Like Horace, he left to more ambitious bards the spirit-stirring drum and the ear-piercing fife. He was, and doubtless knew himself to be, the poet of nature and her aspects; of man and social life; of the foibles and virtues of his age. When he compliments his patrons in what may be termed his laureate productions, he takes for his topics of congratulation or condolence a courtship or a marriage, the loss of royal favour, or a death. Their achievements in Poitou and Picardy are uncelebrated by him.

Yet that during his brief military career Chaucer was no idle or incurious observer of the life in camps, appears throughout the "Knight's Tale," and many passages in his other writings, wherein both the ardour of battle and its image, the tournament, are aptly delineated. The description of the preparation for combat in the lists is worthy to stand very near Shakspeare's better known description of the eve of battle. Compare Chaucer's "Knighte's Tale"—

> "And on the morwe whan the day gan spryng
> Of hors and harneis, noyse and clateryng."

with Shakspeare's Henry V. Act iv. Chorus.

A scholar.—From an allusion in one of his early poems* it has been inferred that Chaucer was educated at Cambridge. Leland, who had good sources of information, says that he was of Oxford, and that he finished his studies at Paris. It is not impossible that each of these universities may in its turn have enrolled the name of Chaucer on its boards. At a time when colleges were little more than grammar-schools it was not unusual for students to migrate from one university to another, in quest of knowledge or attracted

* *Court of Love.*—"My name, alas! my herte why makes thou straunge,
Philogonet I call'd am far and neere
Of *Cambridge* clerk," &c.

by the fame of particular professors. Bologna was second to no school in its day, yet both Dante and Petrarca visited Paris for the purpose of better instruction than their own country afforded. Under whatever auspices, however, or in whatever place Chaucer prosecuted his studies, the extent of his acquirements is testified by his works and the applause of his contemporaries. Besides the lore most attractive to him, the chivalrous bards of the 13th century, and the Provençal minstrels, he was well versed in the theology, philosophy, and scholastic learning of his age. Such science as was then known he had acquired; and it is agreeable to discover that, like Milton, he contributed to the education of the young, since he addressed his "Conclusions of the Astrolabie" to his son "little Lewis." He lived too early, and it was perhaps fortunate that he did so, to be affected by the discovery of ancient manuscripts in the 15th century.

The speaker then briefly surveyed some of the characteristics of Chaucer's diction: to the effect that—it is the language alone of Chaucer which renders him antique or obscure, and even then but partially so: for there still lingers in much of his diction the same vernal brilliance that irradiates his pictures of life and manners.

As regards verse absolutely, and prose partially, Chaucer was the workman who forged the tools with which he wrought. He took the English language indeed as it was used in his time, and as every true poet will do in its best estate at that time. But what *was* the estate of the English language in the 14th century? What was the coin ready-minted to Chaucer's hands? For the learned and all ecclesiastical purposes the Latin was still a living speech; French was generally employed at court, in noble households, and epistolary correspondence. It had but recently ceased to be the language of statute law and legal procedure. With the mass of the people the Anglo-Saxon remained in use, mutilated indeed of many of its inflections, and passing rapidly into that tertiary form which is the characteristic of the English language. From all these elements Chaucer welded together an idiom which retains a portion of each of them: its bones and sinews being Anglo-Saxon; its integuments and complexion, Latin or romantic.

The difficulty of Chaucer's language arises not from any affectation of antiquity on his part, nor from the corruptions of his manuscripts, nor from any total revolution in the English tongue, removing his poems into the region of Middle English or Anglo-Saxon. He is often hard to be understood, simply because his idiom is nearly as much his own creation as the joyous, pathetic, and passionate images with which his writings abound. There is, perhaps in all literature nothing more remarkable than Chaucer's language. It is, strictly speaking, neither a living nor a dead language; it must not be fettered by syntactical rules, nor tried by common usage. Of these peculiarities the condition of the English tongue in Edward III.'s time was in some measure

the cause. In the first place the area of book-language was very limited: while the dialectic varieties of speech were very numerous. English was then imperfectly understood in the Celtic districts of the island: north of the Humber, and in East Anglia, it was encountered by a Danish patois. It was further circumscribed by the general employment of French by the nobles, and of Latin by ecclesiastics. Political motives, indeed, induced Edward to encourage the use of the English tongue in the courts of justice, and among his courtiers and attendants; but there is no evidence of his having been, as his grandson Richard really was, a patron of literature.

Chaucer and Langland are the two principal witnesses for the silent revolution which our language was undergoing in the 13th and 14th centuries. It is curious, that at the time when the author of the "Vision of Piers, the Ploughman," was labouring to re-invigorate our speech with Saxon forms, the author of the "House of Fame," was entering upon his task of enriching it with a foreign vocabulary, and moulding it to a spirit and forms of expression different from either of its original components. Doubtless a similar feeling of dissatisfaction with the existing state of the English language led William Langland to his Saxon archaisms, and Geoffery Chaucer to his French and Provençal innovations. The mightier genius proved himself to be the wiser workman of the two. Langland's poem is studied by philologers alone; Chaucer's writings began a new era in English literature, and his influence has been felt and acknowledged by every successive generation of English poets.

It is still an unsettled question whether Chaucer were acquainted with the literature and learned men of Italy. Sir Harris Nicolas doubts it without sufficient reason, and in despite of Chaucer's own assertions. With the writings of Dante, he was evidently well acquainted, and distinctly quotes from him more than once. The sentence cited by him in the "Wyf of Bathe's Tale," is almost a literal translation from the Italian:—

"Wel can the wyse poet of Florence
That hatte Daunt speke of this sentence;
Lo, in such maner of rym is Dauntes tale:
Ful seeld uprisith by his braunchis smale
Prowes of man, for God of his prowesse
Wot that we claime of him our gentilesse."*

It is not so certain that Chaucer visited Petrarch at Padua, since the statements of Speght and Urry rest wholly on surmises; and it

* Compare "Purgatorio," vii., 121:—
"Rade volte risurge per li rami
L' humana probitate; et questo vuole
Quei che la da, perche da se si chiami."

is doubtful whether the following lines from the "Prologe to the Clerk of Oxenford's Tale," refer to the poet himself, or the original narrator of the story.

> "I will yow telle a tale, which that I
> Lerned at Padowe of a worthy clerk
> As proved by his wordes and his werk
> Frances Petrark, the laureat poete
> Highte this clerk, whos rhetorique swete
> Enlumynd all Ytail of poetrie."

It would be rash to expect that Chaucer will ever regain his position as a national favourite. All that we can claim for him is the recognition of his surpassing worth as an adjunct to the chroniclers of his age, as standing, both by right of time and right of power, in a similar relation to English literature with that of the "all Etruscan three" to Italian, as no less worthy than Petrarch himself of the laurel crown. His vigour and freshness are, like nature herself, perennial; his powers of observation have never been surpassed; his vein of humour and portraiture of manners have been exceeded by Shakspeare alone. He is not only the most conspicuous, but also the sole intellectual representative of England in the 14th century, and his true and lively pictures of its men and manners render the dry bones of its chronicles still capable of receiving form, motion, and life. In his own words, slightly modified—

> "Though he be hoar, he fares as doth a tree
> That blossometh ere the fruit y-waxen be:
> The blossomy tree is neither dry nor dead;
> He feeleth nowhere hoar but on his head;
> His hearte and all his limbës be as greene
> As laurel through the yeare is for to seene."

[W. B. D.]

ANNUAL MEETING,

Thursday, May 1.

THE DUKE OF NORTHUMBERLAND, K.G., F.R.S., President, in the Chair.

The Annual Report of the Committee of Visitors was read, and adopted.—It states that the account of Expenditure for 1855 has been duly examined, and the several items thereof compared with the vouchers; and that the Receipts continue in a satisfactory state, the Annual Contributions being equal to those of 1854, and superior to any former year; while the compositions received from Members, in lieu of future annual payments, have been above the average number. The surplus income beyond the expenditure has

enabled the Managers to invest £600 in the purchase of stock, besides the usual annual additions to the Accumulating Funds of the Institution ; and the property of the Royal Institution, including the house and furniture, the library, &c., and the sums invested in the public funds, now amounts to about Fifty Thousand Pounds. The presence of His Royal Highness Prince Albert on several occasions, and of the Prince of Wales and Prince Alfred at the whole of the juvenile course of 1855-56, was adverted to.

A List of Books Presented (amounting in number to 300 volumes,) accompanies the Report, making a total, with those purchased by the Managers and Patrons, of 743 volumes (including Periodicals) added to the Library in the year.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to Professor Faraday, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year :—

President—The Duke of Northumberland, K.G. F.R.S.
Treasurer—William Pole, Esq. M.A. F.R.S.
Secretary—Rev. John Barlow, M.A. F.R.S.

Managers.

William H. Blaauw, Esq. M.A. F.S.A.
Sir Benjamin Collins Brodie, Bart. D.C.L. V.P.R.S.
Thomas Davidson, Esq.
Warren De la Rue, Esq. Ph.D. F.R.S.
George Dodd, Esq. F.S.A.
Sir Charles Fellows.
W. R. Grove, Esq. M.A. Q.C. F.R.S.
Lieut.-Col. F. Vernon Harcourt, M.P.
Henry Bence Jones, M.D. F.R.S.
George Macilwain, Esq.
Sir Roderick I. Murchison, G.C.S. D.C.L. F.R.S.
Frederick Pollock, Esq. M.A.
Joseph William Thrupp, Esq.
Sir James Shaw Willes, Justice of the Common Pleas.
Col. Philip James Yorke, F.R.S.

Visitors.

Francis Bayley, Esq.
John Charles Burgoyne, Esq.
John Robert F. Burnett, Esq.
Hugh W. Diamond, M.D. F.S.A.
Gordon W. J. Gyll, Esq.
Thomas Henry, Esq.
John Hicks, Esq.
John Holdship, Esq. M.A.
R. Reginald I. Morley, Esq.
Thomas N. R. Morson, Esq.
The Viscount Ranelagh.
Joseph Skey, M.D.
Rev. William Taylor, F.R.S.
Hensleigh Wedgwood, Esq. M.A.
Thomas Young, Esq.

The President nominated the following Vice-Presidents for the ensuing year :—

W. Pole, Esq., Treasurer.
Rev. J. Barlow, Secretary.
Sir B. C. Brodie.
Sir Charles Fellows.
W. R. Grove, Esq.
H. Bence Jones, M.D.
Sir R. I. Murchison.

WEEKLY EVENING MEETING,

Friday, May 2.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Professor Owen, F.R.S.

On the Ruminant Quadrupeds and the Aboriginal Cattle of Britain.

The speaker introduced the subject of the Ruminant order of quadrupeds, and the source of our domesticated species, by some general remarks upon the classification of the class *Mammalia*, and on the characters of the great natural group defined by Ray and Linnæus as the *Ungulata*, or hoofed mammalia.

These are divisible into two natural and parallel orders, having respectively the *Anoplotherium* and *Palæotherium* as their types, which genera, as far as geological researches have yet extended, were the first, or amongst the earliest, representatives of the *Ungulata* on this planet.

The brilliant researches by Baron Cuvier, the founder of palæontological science and the reconstructor of those primeval hoofed animals, from fragmentary fossil remains in the gypsum quarries at Montmartre, were alluded to.

Diagrams of the entire skeletons of the anoplotherium and palæotherium were referred to, in illustration of their dental and osteological peculiarities.

The *Anoplotherium*, with the typical dentition of

$$incisors\ \frac{3-3}{3-3},\ canines\ \frac{1-1}{1-1},\ premolars\ \frac{4-4}{4-4},\ molars\ \frac{3-3}{3-3} = 44,$$

had all its teeth of the same length, and in a continuous unbroken series: this character is peculiar to man in the existing creation. The *Palæotherium*, with the same dental formula as the *Anoplotherium*, had the canines longer than the other teeth, and developed into sharp-pointed weapons; necessitating a break in the dental series to receive their summits in closing the mouth.

The anoplotherium had 19 vertebræ between the neck and

sacrum, viz., 13 dorsal, and 6 lumbar. The palæotherium had 16 dorsal, and 7 lumbar vertebræ.

The anoplotherium had a femur with 2 trochanters, and the fore-part of the ankle-bone, called "astragalus," divided in 2 equal facets. Its hoofs formed a symmetrical pair on each foot. Cuvier has very justly inferred that its stomach must have been complex, and probably, in some respects, like that of the camel or peccari. The palæotherium had a femur with 3 trochanters, an astragalus with its fore-part unequally divided, and hoofs, 3 in number, on each foot. It most probably had a simple stomach, like the tapir and rhinoceros, which, amongst existing animals, most nearly resemble that extinct primitive hoofed quadruped, with toes in uneven number.

Every species of ungulate mammal with an uneven number of hoofs or toes, that has been introduced into this planet since the eocene tertiary period, whether it have 1 hoof on each foot, as in the horse, 3 as in the rhinoceros, or 5 as in the elephant, resembles the palæotherium in having more than 19 dorso-lumbar vertebræ, which vertebræ also differ in number in different genera; 22, *e.g.* in the rhinoceros, 23 in the mastodon, 27 in the hyrax. The typical pachyderms, with an odd number of hoofs, have also three trochanters on the femur, the fore-part of the astragalus unequally divided, and the pattern of the grinding surface of the molar teeth unsymmetrical, and usually crossed by oblique enamel ridges. All the existing odd-toed or perissodactyle mammals have a simple stomach, and a vast and complex cæcum; the horned species have either a single horn, or two odd horns, one behind the other on the middle line of the head, as, *e.g.*, in the one-horned and two-horned rhinoceroses.

Every species of ungulate animal with hoofs in even number, whether 2 on each foot, as in the giraffe and camel, or 4 on each foot, as in the hippopotamus, resembles the anoplotherium in having 19 dorso-lumbar vertebræ, neither more nor less; in having 2 trochanters on the femur, in having the fore-part of the astragalus equally divided, and in having the pattern of the grinding surface of the molar teeth more or less symmetrical. The horned species have the horns in 1 pair, or 2 pairs. All have the stomach more or less complex, and the cæcum small and simple. In the hog the gastric complexity is least displayed; but in the peccari the stomach has 3 compartments; and in the hippopotamus it is still more complex. But the most complex and peculiar form of stomach is that which enables the animal to "chew the cud," or submit the aliment to a second mastication, characteristic of the large group of even-hoofed *Ungulata*, called "*Ruminantia*."

These timid quadrupeds have many natural enemies; and if they had been compelled to submit each mouthful of grass to the full extent of mastication which its digestion requires, before it was swallowed, the grazing ruminant would have been exposed a

long time in the open prairie or savannah, before it had filled its stomach. Its chances of escaping a carnivorous enemy would have been in a like degree diminished. But by the peculiar structure of the ruminating stomach, the grass can be swallowed as quickly as it is cropped, and be stowed away in a large accessory receptacle, called the "rumen," or first cavity of the stomach; and this bag being filled, the ruminant can retreat to the covert, and lie down in a safe hiding-place to remasticate its food at leisure.

The modifications of the dentition, æsophagus, and stomach, by which the digestion in the ruminantia is carried out, were described and illustrated by diagrams.

The speaker next treated of the various kinds of horns and antlers; the manner of growth, shedding, renewal, and annual modifications of the deciduous horns, the peculiarities of the persistent horns, the mechanism of the cloven foot; and the provision for maintaining the hoofs in a healthy condition, were pointed out.

The following were the chief varieties of the ruminating stomach. In the small musk-deer (*Tragulus*), there are three cavities, with a small intercommunication canal between the second and last cavity; the "psalterium," or third cavity, in the normal ruminating stomach, being absent. This cavity is likewise absent in the camel-tribe, which have the cells of the second cavity greatly enlarged, and have also accessory groups of similar cells developed from the rumen, or first cavity. These cells can contain several gallons of water. The relation of this modification, and of the hump or humps on the back, to the peculiar geographical position of the camel-tribe, was pointed out.

The modifications of the ruminating stomach, the discovery of rudimental teeth in the embryo *Ruminantia*, which teeth (upper incisors and canines) have been supposed to characterize the pachyderms; the occurrence of another alleged pachydermal character, viz. the divided metacarpus and metatarsus in the fœtus or young of all ruminants, and its persistence in the existing *Moschus aquaticus*, and in a fossil species of antelope; the absence of cotyledons in the chorion of the camel-tribe, with the retention of some incisors as well as canines in the upper jaw of that tribe; the ascertained amount of visceral and osteological conformity of the supposed circumscribed order *Ruminantia*, with the other artiodactyle (even-toed) ungulata; above all, the number of lost links in that interesting chain which have now been restored from the ruins of former habitable surfaces of the earth—all these and other similar facts have concurred in establishing different views of the nature and value of the ruminant order from those entertained by Cuvier, and the majority of systematic naturalists up to 1840. Thus instead of viewing the *Anoplotherium* as a pachyderm, the speaker, having regard to the small size of its upper incisors and canines, to the retention of the individuality of its two chief metacarpal and metatarsal bones, and to the non-development of horns at any

period of life, would regard it rather as resembling an overgrown embryo-ruminant—of a ruminant in which growth had proceeded with arrest of development. The ordinal characters of the anoplotherium are those of the *Artiodactyla*. On the other hand, instead of viewing the horse as being next of kin to the camel, or as making the transition from the pachyderms to the ruminants, the speaker had been led by considerations of its third trochanter, its astragulus, its simple stomach, and enormous sacculated cæcum, the palæotherian type of the grinding surface of the molars, and the excessive number of the dorso-lumbar vertebræ, to the conviction of the essential affinities of the *Equidæ* with other perissodactyles (odd-toed hoofed beasts).

The primitive types of both odd-toed and even-toed ungulates occur in the eocene tertiary deposits: the earliest forms of the ruminant modification of the *Artiodactyla* appear in the miocene strata. The fossil remains of the aboriginal cattle of Britain have been found in the newer pliocene strata, in drift gravels, in brick-earth deposits, and in bone-caves. Two of these ancient cattle *(Bovidæ)* were of gigantic size, with immense horns; one was a true bison *(Bison priscus)*, the other a true ox *(Bos primigenius)*; contemporary with these was a smaller species of short-horned ox *(Bos longifrons)*, and a buffalo, apparently identical in species with the Arctic musk-buffalo *(Bubalus*, or *Ovibos, moschatus)*.

The small ox *(Bos longifrons)* is that which the aboriginal natives of Britain would be most likely to succeed in taming. They possessed domesticated cattle *(pecora)* when Cæsar invaded Britain. The cattle of the mountain fastnesses to which the Celtic population retreated before the Romans, viz. the Welsh "runt" and Highland "kyloe," most resemble in size and cranial characters the pleistocene *Bos longifrons*. Prof. Owen, therefore, regards the *Bos longifrons*, and not the gigantic *Bos primigenius*, as the source of part of our domestic cattle.

From the analogy of colonists of the present day he proceeded to argue that the Romans would import their own tamed cattle to their colonial settlements in Britain. The domesticated cattle of the Romans, Greeks, and Egyptians bore the nearest affinity to the Brahminy variety of cattle in India. As the domestic cattle imported by the Spaniards into South America have, in many localities, reverted to a wild state, so the speaker believed that the half-wild races of white cattle in Chillingham Park, and a few other preserves in Britain, were descended from introduced domesticated cattle. The size of the dew-lap, and an occasional rudiment of the hump in these white cattle, as well as the approximation to the light grey colour characteristic of the Brahminy race, seemed to point to their primitive oriental source. But the speaker could not regard the pure white colour as natural to a primitive wild stock of oxen. It is now maintained by careful destruction of all piebald calves that are produced by the so-preserved half-wild breeds.

If the blood of any of the aboriginal cattle, contemporary with the mammoth and hairy rhinoceros, still flowed in the veins of any of our domesticated races, he thought it would be that of the *Bos longifrons* transmitted through the short-horned or hornless varieties of the oxen of the mountains of Wales and Scotland.

In conclusion the speaker referred to the subjoined table of the classification of recent and extinct hoofed quadrupeds, as indicative of the progressive extinction of those forms of *Ungulata* least likely to be of use to man, and of the substitution of the ruminant forms, which, from the perfect digestion of their food, elaborate from it the most sapid and nutritious kinds of flesh.

UNGULATA.

Typica.

ARTIODACTYLA*	PERISSODACTYLA†
Anoplotherium	Palæotherium
Chalicotherium	Paloplotherium
Dichobune	Lophiodon
Cainotherium	Coryphodon
Poebrotherium	*Tapirus*‡
Xiphodon	Macrauchenia
Moschus‡	Hippotherium
Antilope	*Equus*
Ovis	Elasmotherium
Bos	*Hyrax*
Cervus	*Rhinoceros*
Camelopardalis	Acerotherium.
Camelus	
Auchenia	
Merycotherium	
Merycopotamus	
Hippopotamus	
Dichodon	
Hyracotherium	
Hyopotamus	
Anthracotherium	
Hippohyus	
Choeropotamus	
Dicotyles	
Phacochoerus	
Sus.	

* Ἄρτιος, *par*; δάκτυλος, *digitus*.

† Περισσοδάκτυλος, *qui digitos habet impares numero*.

‡ *Only those* genera printed in *italics* now exist.

Aberrantia.

TOXODONTIA	SIRENIA
Toxodon	*Manatus*
Nesodon.	*Halicore*
	Rytina
PROBOSCIDIA	Halitherium
Elephas	Prorastomus.
Mastodon	
Dinotherium.	

[R. O.]

GENERAL MONTHLY MEETING,

Monday, May 5.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

C. W. Dilke, Esq.
C. Wentworth Dilke, Esq. and
George Hudswell Westerman, Esq.

were duly *elected* Members of the Royal Institution.

The following Professors were unanimously re-elected :—

WILLIAM THOMAS BRANDE, Esq. D.C.L. F.R.S. L. & E., as Honorary Professor of Chemistry in the Royal Institution.

JOHN TYNDALL, Esq. Ph.D. F.R.S. as Professor of Natural Philosophy in the Royal Institution.

A special vote of thanks was given to the LORD STANLEY, M.P. M.R.I. for his present of a Magneto-Electric Machine, by Watkins and Hill.

The following PRESENTS were announced, and the thanks of the Members returned for the same :—

FROM—

HER MAJESTY'S GOVERNMENT (*by Sir R. I. Murchison*)—Memoirs of the Geological Survey of the United Kingdom : British Organic Remains, Decades V. & VIII. 4to. 1855–6.

Administration of the Mines in Russia—Compte Rendu Annuel, pour 1854. Par A. T. Kupffer. 4to. 1855.

Arnold, Thomas James, Esq. Life Sub. R.I.—The Law Amendment Journal, for April 1856.

Astronomical Society, Royal—Monthly Notices. Vol. XVI. No. 6. 8vo. 1856.

Author—The Great Arctic Mystery. 8vo. 1856.

Bache, Dr. A. D. (the Superintendant)—Annual Report of the United States Coast Survey, 1853. 4to.

Bayley, Francis, Esq. M.R.I.—The Book of Common Prayer, with Notes by Sir John Bayley, Knt. 8vo. 1824.
Memoirs of P. H. Bruce: a Military Officer in the Service of Prussia, Russia, and Great Britain; containing his Travels in Germany, Russia, &c. 4to. 1782.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for May 1856. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for April 1856. 4to.
Bradbury, Henry, Esq. M.R.I.—The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Part 13. fol. 1856.
British Architects, Royal Institute of—Proceedings in April 1856. 4to.
British and Foreign Bible Society—The Bible in Chinese. 8vo. 1856.
Calcutta Council of Education—Report on Public Instruction in the Lower Provinces of the Bengal Presidency, 1852–55. 8vo.
Chemical Society—Journal, No. 33. 8vo. 1856.
Civil Engineers, Institute of—Proceedings in April 1856. 8vo.
Editors—The Medical Circular for April 1856. 8vo.
The Practical Mechanic's Journal for April 1856. 4to.
The Journal of Gas-Lighting for April 1856. 4to.
The Mechanic's Magazine for April 1856. 8vo.
The Athenæum for April 1856. 4to.
The Engineer for April 1856. fol.
Ethnological Society—Journal, Vol. IV. 8vo. 1856.
Faraday, Professor, D.C.L. F.R.S.—Monatsberichte der Königl. Preuss. Akademie, Feb. 1856. 8vo. Berlin.
Franklin Institute of Pennsylvania—Journal, Vol. XXXI. No. 4. 8vo. 1856.
Graham, George, Esq. (Registrar-General)—Reports of the Registrar-General for April 1856. 8vo.
13th, 14th, 15th, and 16th Annual Reports, for 1850–3. 8vo. 1854–6.
Halswell, Edmund S. Esq.—Reports of the Visitors of the County Lunatic Asylum at Colney Hatch, Middlesex. 12mo. 1852–6.
Jackson, Charles T. M.D.—Congressional Report on the Ether Discovery. 8vo. 1852.
Law, Wm. John, Esq. (the Author)—Reply to Mr. Ellis's Defence of his Theory on the Route of Hannibal. 8vo. 1856.
Lewin, Malcolm, Esq. M.R.I.—Speech of the Earl of Albemarle on Torture in the Madras Presidency. 8vo. 1856.
Mitchell, Mr. (the Publisher)—The Crusades: a Lecture, by Viscount Cranborne. 8vo. 1856.
Murray, Hon. Henry A. M.R.I. (the Author)—Lands of the Slave and the Free: or Cuba, the United States, and Canada. 2 vols. 16mo. 1855.
Newton, Messrs.—London Journal (New Series), May 1856. 8vo.
Novello, Mr. (the Publisher)—The Musical Times, for April 1856. 4to.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1856: Heft 2. 4to. Gotha, 1856.
Photographic Society—Journal, No. 41. 8vo. 1856.
Pollock, W. Frederick, Esq. M.R.I.—The Works of Abp. Grindal, Abp. Sandys, Bp. Pilkington, Bp. Ridley, Bp. Hooper, Archdeacon Philpot, R. Hutchinson, and W. Fulke. (Published by the Parker Society.) 8 vols. 8vo. 1842–3.
Prosser, John, Esq. Life-Sub. R.I.—Observations on a General Iron Railway, with Plates and Map. 8vo. 1823.
Thirteen Letters from Sir I. Newton to John Covel, D.D. 8vo. 1848.
The Suppressed Letter to G. Canning. 8vo. 1818.
Quinton, J. R. Esq. (the Author)—Painless Tooth Extraction by Congelation. 16mo. 1856.
Redpath, Leopold, Esq. M.R.I.—Paradise Lost. A Poem, in Ten Books. The Author, John Milton. London, Printed by S. Simmons. 4to. 1669.
Royal Society of London—Proceedings. No. 20. 8vo. 1856.
Society of Arts—Journal for April 1856. 8vo.

South, Sir James, F.R.S. M.R.I. (*the Author*)—Letter to the Fellows of the Royal and Royal Astronomical Societies. 8vo. 1856.
University College, London—Annual Report, 1856. 8vo.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, Jan. und Feb. 1856. 4to.
Vincent, B. Asst.-Sec. R.I.—Dr. H. Winter, Litergeschichte der Deutschen Sprach Dicht und Rede-Kunst. 8vo. Leipzig, 1829.
Dr. J. W. Schäfer, Grundriss der Geschichte der Deutschen Literatur. 8vo. Bremen, 1850.
Webster, J., M.D. F.R.S. M.R.I.—Reports of Bridewell and Bethlem Hospitals, &c. 8vo. 1855.
Williams, Thomas, M.D. (*the Author*)—On the Mechanism of Aquatic Respiration, and on the Structure of the Organs of Breathing in Invertebrate Animals. 8vo. 1853.
Yorkshire Philosophical Society—Proceedings. Vol. I. 8vo. 1855.

Stanley, The Lord, M.P. M.R.I.—A Magneto-Electric Machine, by Watkins and Hill.
Pearsall, Mr. T.—Cast of a Shell in Limestone, from Malton, Yorkshire.

WEEKLY EVENING MEETING,

Friday, May 9.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Henry Bradbury, Esq. M.R.I.

On the Security and Manufacture of Bank Notes.

The many processes connected with the manufacture of Bank Notes invest the subject with deeper interest than the mere importance attached to them as symbols of commercial confidence. To enter into the consideration of these processes would require a lengthened work. The object is rather to direct attention to the consideration of that which is the most important feature in their manufacture, namely the engraving, because upon it their security in the eyes of the public mainly depends. It may perhaps be asked,—What occasion is there for discussing this question? It is replied: that forgery is on the increase; that difference of opinion exists as to the soundest method to be employed for obviating it; that facilities are growing up to assist forgery; and that, further, there is a tendency to employ that method which, in reality, is most exposed to the operations of the forger. If, indeed,

we could be sure that the advance of genuine Art must ever distance that of the spurious—if the success of the forger were to stand in an inverse ratio to the genius of the artist—and if we were to ignore the wonderful skill and ability of the so-called uneducated classes—then indeed this subject would lose a considerable portion of its interest, and its technicality would be deprived of its moral importance.

Had, however, the Report of the Committee * (though sitting upon this question so far back as 1819) been acted upon, it appears that this state of things could never have arisen; for they arrived at those sound conclusions which are perfectly applicable at the present day. This Committee was organised in consequence of the rapid increase of convictions for the circulation of Bank of England Note forgeries. Juries began at last to feel a reluctance to visit with capital punishment a crime, for the prevention of which no proper precautions seemed to have been taken. The fact, also, that forged notes had passed undetected the scrutiny of the Bank Inspectors—determined the Council of the Society of Arts to take this step. The point for their especial consideration was to determine the means within the compass of Art, not so much totally to prevent the forgery of Bank of England Notes, (for that was obviously impossible,) as to elicit means of detection by increasing difficulty of imitation.

This Report was one of great value, and is in these days still further capable of extension, with the aid of a nice judgment, joined to artistic advantages which can now be commanded, to obviate many of the difficulties which originally beset this subject.

The main feature, then, of the note, the Engraving, and its security, has been proved in practice as well as deposed to by artists of eminence, to depend upon the Vignette. The higher the quality of the artistic impress, therefore, which the vignette carries—the purer and severer the tone conferred upon its execution—the greater the security of the note. This artistic impress might be still further extended to the whole face of the production.

The great value of the vignette consists in this, that it is the uncounterfeitable seal of the note;—uncounterfeitable—because, though it may be imitated, its individuality cannot. This is illustrated by comparison with a picture; it always conveys the style of the artist: his composition is known—his colour—his *chiaroscuro*—which the component parts of all works of Art have—a special individuality, not to be obliterated from memory and which no copy can possess.

However similar, there is a difference in the human countenance;—however similar, there is a difference in handwriting. If then any

* Report of the Committee of the Society of Arts, on the Mode of Preventing the Forgery of Bank Notes, &c. London, 1819.

number of the most eminent engravers were to endeavour to copy each other,—there would be sufficient evidence on casual examination to detect it. In a rivalship between them they might produce a work of similar beauty and general effect, but the difference of manner would be obvious to the commonest observer, and not only would the forgery be discovered, but the hand that had executed it would be identified. The eye of the Banking clerk, or the man of business, would soon become expert at this kind of Fine-Art reading. This was proved in the case of the Plymouth Bank half a century ago; their bills were forged,—their notes were not, simply on account of the vignette. When the vignette was added to the bills, the forgeries ceased.

Of the various methods of engraving, the choice more especially lies between that of *intaglio* and *surface-engraving*, between steel and copper-plate, or letter-press and wood-engraving; of the two, the first ranks pre-eminent, both for its beauty and adaptability to the production of Bank Notes. Years ago, objections might have been validly urged against this method. When the cost consequent upon the engraving of plates as they wore out counterbalanced the advantages gained, and moreover, the often changing the individuality expressed upon the production, negatived its highest quality, the plan presented obstacles not easily surmounted in extensive practice; but now Science enables us to overcome the impediment, and the Electrotype gives from one original an infinity of reproduction, with little more than nominal outlay.

With regard to letter-press or surface-engraving, its power is too limited in its effects to realise a high standard of artistic finish; while even cheapness, combined with rapidity of production, is not sufficient to counterbalance the absence of that continuous and unalterable individuality which should be sought for as the distinguishing feature of the Bank Note. In stating this opinion, weight of contrary evidence has to be contended against, inasmuch as the Notes of the Bank of England, as also those of the Bank of France, Bank of Belgium, and Bank of Russia, are printed from surface-engraved plates: for the reasons assigned, such evidence cannot countervail the immeasurable superiority of *intaglio* engraving for the main object desired,—namely, security. There is, however, one application—and that of considerable magnitude—to which this mode of printing has become of late more immediately applicable. It is used in the case of receipt-stamps in this country, and in that of postage-stamps in France. The main proposition on this subject is, that the union of Art, in which this country has been deficient, with Manufacture in which she is unrivalled (the vision of which is dawning in the distance), would place England in advance of every other nation.

In the engraving of the Bank Note two principles are involved

for consideration: first, the *simplicity* of its design, the purity of which is the gauge of its perfection; secondly, the combination of vignette-work with intricate engine-work: both these principles possess high claims to the attention of Banking authorities as security against forgery—the one on account of the difficulty of mechanical imitation—the other on the principle of the certainty of a first-sight recognition. Simplicity of design, when it amounts to the character of high-class Art, is much the best with regard to the issue of National Notes. Anything which addresses the mind is more clearly distinguishable than that which addresses the eye—and where variety of pattern and freaks of ornament distract the attention, it is put more within the field of the imitator than when he has to contend with an ideality—for which neither his education nor his pursuits are likely to fit him.

By way of illustrating this principle an imaginary Note of the Bank of England is here submitted, because that establishment stands proudly and preeminently at the head of the monetary transactions of the whole world, and because its Notes are more familiar.

The Note is as simple as a National Note can safely be. Its attributes are individual unity, if not beauty; simple and salient features, with due prominency of numerical value. The general character has a sort of medieval cast—it has been chosen, partly because it is totally different from the cursive style in common use, and is also in accordance with the revival of that style in the present day.

The national characteristics are boldly expressed and displayed: attempt has been made to extend the artistic ideality of the Vignette —which is emblematic of the Nation—to the whole production. Breadth of design and unity of purpose have been sought for. Care has been taken in the introduction of the ornamentation, that it should not assume a position so prominent as to weaken the artistic effect, but rather serve as an auxiliary to Art. An additional and novel feature is thus conferred upon the writing, and all the points (and this should be borne in mind) subserving their particular purposes, contribute to the general harmony of the whole. Objections may be raised, that it is too architecturally bold—too florid in display—but it appears self-evident, that the nobler the character of a Note, the less it would enter into the comprehension of the forger—and even if some were not sensible of the difference between a fine original and a bad copy, that is no reason why others of better judgment should be precluded.

The Bank of England Note has always been characterised by simplicity, but carried to an extreme in the opposite direction, the same general design having been preserved from the issue of their first note. The objection is, that its simplicity is *too* simple,—not bearing upon the face of it those features which characterise the


JANUARY. 1. 1856.
J.L. No 12345.
JANUARY. 1. 1856.
J.L. No 12345.
Bank of England.
I Promise to pay the bearer on demand
the sum of TEN POUNDS.
For the Governor & Company of the
BANK OF ENGLAND.
TEN
Specimen. Bank Note.

true Art-point. The vignette is a specimen the reverse of that which has been advocated; it is alike deficient in conception and execution. Surface-printing having been chosen as the medium, the Bank authorities were restricted in the application of their Art. The great aim of the Bank has been to secure simple identity and ready recognition through the excellence of the paper, known by its peculiar colour, by its thinness and transparency, as well as by its feel, crisp and tough, patent to the sense of touch alone. The basis of its security to the public rests upon its paper. It is supposed to be unmatchable. As successful imitations of this paper, however, have been made abroad, and passed in this country, too much reliance ought not to be placed upon this superiority of the paper. Again, the fallacy of its security consists in the extreme facility it affords for reproduction by hand, apart from reproduction afforded either by the Anastatic or Photographic processes. From 1837 to 1854, these notes were printed from steel plates, reproduced by the Siderographic or Transfer process; at the commencement of 1855, a change took place in the production of the notes by the substitution of surface-printing from electrotypes for steel-plate printing. A variation was then made in the form of the old note, by adopting an engraved signature instead of a manual one—the object being still further to strengthen the identity of recognition.

The note of the Bank of France for 100 francs is a fair specimen of surface-printing: but its inferiority of design indicates that it has been adapted to its limited capabilities, and in this case is liable to those objections which are assignable to our own. The note however for 1000 francs is less exposed to the objections raised against the note for 100 francs, the difficulties of copying by hand being very great. These notes are printed upon both sides at the same time, effected by pulling an impression on the tympan itself before pulling each impression of the note: the reason for so doing is, that when the impression is pulled upon the face of the note, the paper receives two impressions at the same time,—the one, on the face of the note from the printing plate,—the other, on the reverse of the note, transferred from each impression on the tympan pulled previous to the genuine impression. The two impressions must necessarily register. This course has been pursued with the idea that perfect register of the two printings is a good gauge for detecting imitations. There is some reason in this, as it is a most difficult and tedious operation, requiring consummate skill on the part of the workman. It is said that not more than three hundred impressions are printed daily at one press—this does not speak much for economical production.

Simplicity of design, however, is not to be advocated as solely applicable to a note in all instances. It is more especially suited for national notes, because immediate recognition should be one of their essential features; but for provincial or local notes, not having

so wide a commercial circulating range, the complex note has peculiar advantages; the display of national emblems—ideal impress —and intuitive recognition being here unnecessary; one merit in particular which it possesses, is that it is more available to represent small amounts, and for circulation among the humbler classes; for an obvious reason, — the repetition of the amount represented, affording to the holder a more continued appreciation of its value.

Art being the principle of a simple note; Mechanics, or engraving by machinery, is that of a complex. This is performed in two ways; relief and medallion-work, guilloche or rose-engine-work. The one represents models on flat surfaces: the other, lines, straight, waved or curved, circles, ellipses, parallel or intersecting, resulting in a particular effect. Different results are sought in the combinations produced from such machines: some prefer figures to appear white on black; others prefer the reverse, the natural one, black on white. The latter is free from the confusion presented in the former; its effect is more beautiful, and affords a much greater degree of detection.

In the instruments used for the execution of these designs, (being the laboriously-perfected construction of F. J. Wagner, K.-H. Mechanicus, of Berlin), the adjustment admits such variety, that even allowing the forger to be possessed of a similar instrument, the chances against hitting on the several requisites to produce any particular pattern are infinite. It may admit of a doubt, whether their very intricacy, and the want of any prominent part to impress the attention, would not allow even a general resemblance to pass in the hurry of business. It is possible, however, to combine even within this intricacy of pattern and ornament an idea of that simplicity and singleness of recognition above mentioned.

This Note, for consecutiveness of argument, is also dignified with the name of the Bank of England. Even in this, Art may be made to hold an important place; for idea in design can be seen in its impress, and thus almost the same, if not greater, from its mechanical construction, amount of security may be guaranteed, previously stated to have been the object of ***simplicity***. It possesses unity of parts and purposes in design which tell much against the imitator. It is organised, as combining beauty with a business appearance. This small and convenient note can be divided into four parts for postal security.

Its principle is essentially the same as that in the simple note—having its own individuality. Too much ornamentation is apt to bewilder, nay, misguide—too little is apt to abuse: but a proper combination of a certain amount of elaborate mechanical work, properly balanced to meet the effect of the design, will also combine beauty with security, with hardly less facility of recognition.

The feasibility of this has been lost sight of—in fact, it was not *formerly* attainable—nor was it practicable until the Electrotype

became a recognised agent in practical reproduction. By this power, surfaces of any given dimensions can be multiplied without reference to the quantity or delicacy of the work: not so by the mode of reproduction known as the Siderographic process for transferring objects engraved on steel to steel. This latter is certainly capable of reproducing to infinity, but with this important difference, that the smaller the subject the greater success in the transfer. While the fact of requiring retouching is an additional objection not at all applying to the Electrotype. It will be thus understood that so long as a process of reproduction was adhered to, only capable of transferring small subjects or fragments of designs, and in every case requiring to be retouched by the graver, so long must such kind of note be built up as it were piece by piece, and thereby perpetuate the barrier between the Art-ideal and the Art-mechanical.

Since the discovery of the Electrotype, efforts have been made in several cases to apply its perpetuating power to the reproduction of copper engravings generally,—with no better result, however, in every case than signal failure. The average number of impressions from one plate rarely reached 500—the electro-copper, too, spread from the pressure of the printing-press—and, in addition, from its softness, even curled round the cylinders. Too little attention was paid to the science of electro-deposition; and failure arose from a want of confidence in its power, and want of energy in investigation. At the present time, partly owing to the perfection at which the process has arrived, and partly owing to an additional new agency brought into co-operation, an experiment was made a few months ago, under every possible disadvantage, to re-establish in this country (for it has long been in successful operation at Vienna) its practical, as well as its economical, adaptability. This experiment was made upon the Bank Note of the National Bank of Brazil amounting to 1,200,000 notes, of very elaborate work, which have been printed from Electrotype plates: at the completion of this work, the means for future production existed for printing as many million notes more as thousands have already been printed—and, with this certainty, that the last note printed shall be identical with the first. The electro-plates in this instance, partly owing to the increased hardness in the copper deposited, and partly owing to a particular method of treatment, have yielded on an average 1600 perfect impressions—and experiments are in course of operation for increasing this to between 3000 and 4000. The establishment of this fact renders it almost imperative that greater attention should be given to the character of a note. If at the time that Art was more or less in its infancy, every endeavour was made to elevate the character of the Bank Note, and reports of scientific and practical men were sought for, and their suggestions attended to; *à fortiori*, now that Science has done so much, and is capable of

doing more, the Art-question of this subject ought to hold an important place in its consideration. The peculiar advantage of the Electro over the Siderographic process will be apparent, seeing that the Electro copies the whole surface—and furthermore, it copies the exact state of the plates, without requiring the aid of scraper or burnisher, or that careful retouching and deepening of lines so indispensable to the success of Siderography.

The term Siderographic has been made use of in connection with Bank Note printing. This is a process for transferring figures from steel to steel, and thus multiplying the number of plates to be printed from. It is one of exceeding simplicity; but, as in most cases of the kind, however simple it may appear, it requires more than ordinary skill to effect successfully that little of which it is capable. It consists in taking up an engraved plate upon a roller of softened steel, a combination of rolling and pressure. The engraved subject thus stands in relief upon the roller. Identical plates are thus said to be produced. The objections are threefold,—First, every subject so transferred must be subsequently retouched by the graver. Secondly, its transferring properties are limited to very small subjects. Thirdly, and as a consequence, it is inapplicable to the manufacture of complex notes.

Siderography has been in use for thirty years or more in America; in fact, it was the invention of an American: but the Notes of the United States Government are not, and never have been—beautiful as their execution is—carried out after the principles advocated above. Referring to the American Notes, it is a curious fact that America being divided into so many States, and each State being represented by a different note, forgers did not think it worth their while to imitate, and therefore concocted notes of their own.

It may prove interesting to supply some information concerning the Bank Notes of other countries. They carry on the face of them the absence of all those qualities which have been insisted upon, and actually in many instances appear to offer a premium to forgery. The aim would appear to have been to make them as ugly as possible, without affording them any counterbalancing security. Individual figures may be well executed—mechanical workmanship may also seem to be well done—but they are utterly destitute of leading ideas and harmonious properties—the true attributes of a Note.

The Notes of the United States present engraved specimens of the highest order, but the subjects selected seem irrespective of their purpose, and the multiplicity of the figures distracts the attention. Again, there are the Austrian Notes. These fall considerably below the standard of American excellence. Vague in design, coarse in execution, the broken character of their design and the inconvenience of their shape render them unfitted for their purpose. Prussian Notes too partake of the same objections—

[illegible] Specimen of a [illegible] Bank Note

Bradbury & Evans, Bank Note Engravers & Printers, Whitefriars, London.

preserving, in many instances, the appearance of fancy stationery. The Russian Notes might well be adopted for merchandise labels. To particularise the characteristics of all the different foreign notes would only become tedious, while placing them in the same category.

If, however, the Notes of these States, betraying such defects, have in themselves upon the face of them an endeavour to perpetuate something of National Art, it must be plain that this country ought to be willing and able to effectuate a prototype of superiority. Great reliance for security is placed upon a combination of processes, and the greater the number employed in their construction,—the more different the effect resulting from each,—the more difficult it is supposed for the power of a single forger to embrace the exercise of the whole. This, however, is a fallacious dependence, for the confusion created tends to less particularity of observance upon the part of the public.

Thus much said upon the Manufacture of the Bank Note, the question of Security arises, a question most difficult of solution. For two reasons : on the one hand, because it is so generally admitted that what has been executed by one individual may be copied by another; on the other, because it is not in the nature of things that a person who cannot read, should be protected from imposition by the most clumsy forgery. Another reason might be added, the general facility afforded by Science, not merely for the reproduction of one special object, but of almost everything. It is not logical to suppose that while Science helps the producer, she withholds her help from the imitator, and this point is the main one to be considered in the security of the Note. The first reason cannot be substantiated; while the second is disposed of, on the grounds of the general spread of education. What is to be considered is, what is the nature of the so-called scientific facilities, and what are the steps to be adopted in the shape of counter-foils; for while Science does help the imitator, she also comes again to the aid of the producer.

It might be questioned whether any person coming forward to explain how many reproductive processes existed, and to what extent they could be carried, was exercising a privilege consistent with discretion. Such a method of proceeding, though new, is certainly capable of more good than evil; for the more light there is thrown upon the subject, and the more imitators perceive and understand that the eye of genuine Science is upon them, the more fearful they will be of venturing on spurious manufacture that will certainly eventually be detected.

The Anastatic process has been on several occasions brought prominently forward, with such promises, that its powers appeared of a dangerous character, competent to effect an unprincipled object. It professes that copper-plate and other engraving, old or new, ancient or fresh, can be transferred to plates of metal, and reprinted as fac-similes without re-engraving. In such a manner it

is supposed to be able to copy Bank Notes. The process certainly has great claims to utility, if confined to its legitimate sphere; for instance, when within the short space of ten days, 200 sets of fac-similes of the great Austrian Map of Russia and Turkey, for the use of the generals and officers of our armies in the East, were reproduced from 21 original copper-plate engravings, printed in Vienna in 1829. The difference however between the nature of the engraving that characterises a map, and that which characterises (or which *ought* to characterise) a Bank Note, should be considered. The work in the one (the Note), whether it be complex or simple, ought to be sharp and clear, whereas in the case of the Map (and especially in the use for which it was required in the instance specified) it mattered little whether the lines transferred possessed great nicety of sharpness or not. The operation of effecting a transfer requires immense pressure, producing a flattening or spreading of the lines. In copper-plate printing, the ink lies upon the surface of the paper, not a transparent film as in surface-printing, but as a body; the body naturally having a greater tendency to spread, the film a less. Therefore the best and simplest way to meet the supposed danger of the Anastatic is to adopt copper-plate and very fine work—as surface-printing with open-work only favours the transfer.

If the Anastatic process thus lays claim to the ingenuity of an exhausted art, Photography appears as an infant science. When Photography made its first appearance, experiments were instituted in order to ascertain how far it might be made subservient to forgery. Copies of Bank Notes were certainly produced by one or other of the branches of this art—but not to an extent to be considered dangerous. The copies were imperfect—there was a loss of sharpness; an absence of reality; a want of printed effect. The colour, too, instead of being black as in printing, was a sort of brown sepia. Another failure was in the representation of the water-mark, which appeared indeed as if it were really existing, but it was found impossible to give the peculiar effect of the water-mark produced by reflection and transmission. Up to this point then there was little to fear. Since that time Photography has made great advances; and there exists a process which is capable of producing a more serious result than that previously obtained; namely, a printing-plate.

In cases where the engraving on a Note by reason of coarseness of character (such being exemplified more in letter-press than in copper-plate) is exposed to the reproductive powers of the Anastatic or the Photographic processes, and such mode of engraving (viz., the letter-press) must be from economical or other motives followed, then the antidote (at least, that in common use) consists in the adoption of printing in different colours. For instance:—Suppose some words or design elaborately engraved to be first printed in red on

the white paper; and then the Note itself to be printed over this in black—we should have a mixture of black print over red. In attempting to take an Anastatic or Photographic copy of this compound note, we shall have one printing-plate as the result: that is to say, that which was printed first in red, and that which was printed afterwards in black upon that red, is produced intermixed upon one plate, and whatever printing colour you apply to that plate for the purpose of producing an impression, you have the result of the two impressions in one colour; and the point, wherein lies the difficulty, is that the separation of the two printings first referred to, in different colours, cannot be effected without destroying the combined transfer. The more elaborate and artistic the second working in colour is rendered, the more the difficulty of dissection is increased. Again, another reason for employing artistic and elaborate work, is to thwart the efforts of the forger, supposing him to make use of the transfer for the purpose of engraving by hand, instead of using the chemical transfer. Also: if instead of taking those colours that are copyable by the agency of light, colours are selected that are *not* copyable, we shall have a result, which, though not precisely the same, presents difficulties equally insuperable; provided the work be of that character which is produced by the rose-engine-work: for, if these colours are *not* copyable by this agency, and those colours represent work which cannot be copied on the score of mechanical imitation, the forger stands in the same difficult predicament as before. To these points the attention of Banking authorities has been already awakened, as the adoption of it, in whole or in part, has become a prominent feature in commercial securities.

With regard to these processes, admitting that they are, as they really are,—the latter especially,—dangerous where the execution is indifferent in character; or, on the other hand, that they are *not*, or that they are merely put forward to subserve a business purpose,—in either case they should not be disregarded. If no notice be taken of them by bankers, if forgers perceive that increased facilities for copying are not met by increased efforts to defeat them, such indifference only gives encouragement for cultivating forgery as a science.

Casual mention only has been made of the water-mark, not regarding it as unimportant, but secondary to the main consideration; for a successful copy of a good engraving upon a spurious water-mark is more likely to impose upon the ignorant, than an inferior engraving upon the genuine water-mark. If then the water-mark were backed by the excellence of Art and engraving, it might safely be asserted that the Bank Note was unforgeable. But it may be said, the paper thus alone furnishes an absolute security, why make so great a parade of Art, design, engraving, &c., in its manufacture?—Because, whatever internal security exists in the paper, tends rather

to the security of the Bank.—There should also be a *primâ facie* security for the Public; and if both are applied, they re-act upon each other, and the Note is perfect. Some foreign Banks adopt the plan of having different coloured papers for the notes of different denominations of value; how far this is an advisable plan admits of question, for in addition to the fading of colour, the circumstance of receiving a piece of coloured paper is apt to induce a false security, assuming without inspection that the document is of a certain supposed value.

Having shown that production and reproduction react upon each other, the question is, according to what plan the Bank Note should be manufactured? Let Art be impressed on the Note.—Let ingenuity of design, executed by a hand whose genius would at once indicate its authority, be adopted so far as is consistent with a commercial purpose.—Let that mode of printing be employed which alone can render the delicacy and force of an Art-subject. If this course be followed, the greatest possible amount of difficulty would be placed in the way of reproduction by hand; the two scientific processes referred to cannot be brought to bear against it—and further, which should be another grand object, this Art-education of the people would eventually teach them prudence in distinguishing the genuine from the false. Lastly, if this should eventually be found insufficient to secure the desired result of unforgeability, then it should be imperative on the Government to resolve this question, and offer a reward, as has been done in other great public questions, for its solution.

[H. B.]

WEEKLY EVENING MEETING,

Friday, May 16.

Sir Charles Fellows, Vice-President, in the Chair.

Dr. A. W. Hofmann, F.R.S.

On the Chemical Type: Ammonia.

The great laws which govern chemical combination, have been mostly recognized and elaborated by the study of *mineral compounds*, the examination of which at a very early period attracted the atten-

tion of inquirers. It was only much later :—in fact, at a comparatively recent epoch, that the *vegetable* and the *animal world* were drawn into the circle of chemical observation. The progress made in the study of *vegetable* and *animal* substances was based, in the commencement at least, entirely and exclusively upon the knowledge which chemists possessed of *mineral* bodies. The experience, the ideas, gained in the examination of *mineral* substances, reflected themselves, if I may use this expression, in whatever views were brought forward regarding the nature of *vegetable* and *animal* compounds. *Organic Chemistry* was but a reproduction, in another form, of *Mineral*, or *Inorganic Chemistry.*

This aspect, however, of the relative position of the two departments of the science is rapidly changing. The amount of material accumulated by the indomitable perseverance of so many cultivators of Organic Chemistry, (a chaotic and almost inaccessible labyrinth, but a few years ago,) is rapidly assuming shape and order. The study of *organic* bodies has led to the observation of general laws, which could have never been discovered by the examination of *mineral* substances alone, but which begin to react in a most powerful manner upon our ideas regarding the constitution of these very *mineral* substances. The progress of our knowledge of *organic* bodies has opened new points of view, from which the constitution of *mineral* substances appears to us in a brighter light, in a simpler and more intelligible form. In one word, Organic Chemistry is beginning to repay, and I venture to say, with interest, the debt of gratitude which it owes to her elder sister, Mineral Chemistry.

It is my task, this evening, to bring under your notice some especial examples in elucidation of the idea which I have endeavoured to delineate to you. Illustrations of this kind might be taken from widely different departments of the science. In consequence of special studies and predilections of my own, I have selected as material of illustration a class of substances of which the well-known compound Ammonia is the type.

The four elements—Nitrogen, Phosphorus, Antimony, and Arsenic, although essentially differing in many of their physical properties, exhibit nevertheless an extraordinary similarity in their chemical character, and especially in their combining tendencies. With oxygen these four bodies produce teroxides and pentoxides which, in combination with water, have all decidedly acid properties.

Nitrous Acid . . .	NO_3	Nitric Acid	NO_5
Phosphorous Acid .	PO_3	Phosphoric Acid . .	PO_5
Antimonious Acid .	SbO_3	Antimonic Acid . .	SbO_5
Arsenious Acid . .	AsO_3	Arsenic Acid . . .	AsO_5

The latter acids, moreover, appear to be all tribasic ; in phos-

phoric and arsenic acids the tribasic character is well marked; with antimonic acid it is less pronounced; and nitric acid is generally considered as a monobasic acid: but the progress of science will, I have no doubt, confirm our suspicion that the nitrogen-acid is likewise of a tribasic character. The chlorides and bromides, corresponding to the oxides of nitrogen, phosphorus, antimony, and arsenic, also exhibit, within certain limits, similar analogies.*

Again, these four elements unite with hydrogen, and the compounds thus produced have a similar composition; they are all terhydrides.

Ammonia	NH_3
Phosphoretted Hydrogen . . .	PH_3
Antimonetted Hydrogen . . .	SbH_3
Arsenetted Hydrogen	AsH_3

So far the analogy appears to be complete. Extraordinary discrepancies, however, are observed in the properties of these hydrogen-compounds, for although they are all gases at the common temperature, although they all possess a marked odour, and are more or less inflammable, we find that ammonia is *soluble in* water, imparting a *strongly alkaline character* to this solution; while the three other compounds, phosphoretted, antimonetted, and arsenetted hydrogens are *insoluble* in water, and *without the slightest alkaline reaction.* Again, ammonia when coming in contact with acids, absorbs these bodies with the greatest avidity, producing a series of well marked, mostly crystalline, compounds, which are called salts of ammonia, or ammoniacal salts, and of which sal ammoniac and sulphate of ammonia are familiar illustrations. Antimonetted and arsenetted hydrogen, on the other hand, have never been combined with acids; and in the case of phosphoretted hydrogen, only one salt-like compound, the hydriodate of phosphoretted hydrogen is known, which latter certainly presents considerable analogies with the salts of ammonia.

Sulphate of Ammonia	NH_3, HSO_4
Hydriodate of Ammonia	NH_3, HI
Hydriodate of Phosphoretted Hydrogen .	PH_3, HI

The want of similarity observed in the general characters of ammonia and the hydrogen-compounds of phosphorus, antimony, and arsenic, has always been an obstacle in the way of considering the four elements in question as members of the same natural family.†

* For the purpose of illustration specimens of *Nitrogen*, *Phosphorus*, *Antimony*, and *Arsenic*, and of their oxides, chlorides, and bromides were upon the table.

† The preparation and the principal properties of *ammonia*, *phosphoretted*, *antimonetted*, and *arsenetted* hydrogen were experimentally exhibited. The

The modern progress of Organic Chemistry has removed those difficulties.

Organic Chemistry deals with compound molecules, consisting of carbon and hydrogen, occasionally associated with nitrogen and oxygen. These compound molecules, often called compound radicals, simulate the deportment and exercise the functions of elementary substances. One of the most familiar illustrations of organic radicals is the radical Ethyl, consisting of four equivalents of carbon, and five of hydrogen, $C_4H_5 = E$, and which chemists assume to exist in *alcohol* and *ether*, the derivation of which from water becomes obvious by a glance at the following formulæ:—

$$\text{Water} \quad . \quad . \quad . \quad . \quad \left\{\begin{matrix} H\,O \\ H\,O \end{matrix}\right.$$

$$\text{Alcohol} \quad . \quad . \quad . \quad . \quad \left\{\begin{matrix} E\,O \\ H\,O \end{matrix}\right.$$

$$\text{Ether} \quad . \quad . \quad . \quad . \quad \left\{\begin{matrix} E\,O \\ E\,O. \end{matrix}\right.$$

Alcohol may be regarded as water, in which *one* equivalent of hydrogen is replaced by ethyl, *ether*, as water, for the *two* hydrogen-equivalents of which ethyl has been substituted. The general characters of these three compounds greatly differ from one another; but some of the fundamental properties of water, its neutral character for instance, are retained in the two substitution-products.*

Recent researches have proved that in ammonia likewise the hydrogen-equivalents are replaceable by ethyl. Three new compounds are thus produced, which have received the names *ethyla-*

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manufacture of ammoniacal salts was, moreover, illustrated by a series of painted diagrams, and a set of large and beautiful specimens furnished by Messrs. Simpson, Maule, and Nicholson, Kensington. Ammonia was evolved by the action of lime upon sal-ammoniac, phosphoretted hydrogen by that of hydrochloric acid upon phosphide of calcium, antimonetted and arsenetted hydrogen lastly, by introducing antimony- and arsenic-solutions into flasks from which hydrogen was being evolved. Cylinders were filled with the four gases over mercury. The phosphoretted hydrogen evolved proved to be spontaneously inflammable. Antimonetted and arsenetted hydrogen were inflamed by a taper, and ammonia was shown to be capable of combustion by directing the current of the gas through a gas-flame. It was demonstrated that phosphoretted, antimonetted, and arsenetted hydrogens are *not* absorbed by water or even acids, and that they exhibit no alkaline reaction with vegetable colours; whilst ammonia is absorbed by water and acids, and possesses the character of a strong alkali.

* Specimens of water, alcohol, and ether placed in juxta-position. Ethyl-gas, obtained by the action of zinc upon the iodide of ethyl in a strong copper digestor, exhibited and burnt.

mine, *diethylamine*, and *triethylamine:* and the composition of which is illustrated by the following formulæ :—

$$\text{Ammonia} \quad . \quad . \qquad N\begin{cases}H\\H\\H\end{cases}$$

$$\text{Ethylamine} \quad . \quad . \qquad N\begin{cases}E\\H\\H\end{cases}$$

$$\text{Diethylamine} \quad . \qquad N\begin{cases}E\\E\\H\end{cases}$$

$$\text{Triethylamine} \quad . \qquad N\begin{cases}E\\E\\E\end{cases}$$

The three ethylated derivatives fully retain the fundamental character of ammonia ; they are powerful bases, capable of uniting with the acids, and of forming very definite, well-crystallizing salts. Owing to the diminution of volatility with the progress of ethylation, the ethylated bases appear to be even more powerfully basic than the type itself. This development of basic power, as will be presently seen, deserves especial consideration. The substitution of ethyl for hydrogen presents no difficulty, it may be effected by several methods, one of the commonest processes consisting in the action of iodide of ethyl upon the body to be ethylated. Thus ammonia and iodide of ethyl produce ethylamine and hydriodic acid, which unite and give rise to the formation of hydriodate of ethylamine.*

$$N\begin{cases}H\\H\\H\end{cases} + E\,I = N\left.\begin{matrix}E\\H\\H\end{matrix}\right\}H\,I,$$

In consequence of the ethylated derivatives of ammonia retaining the basic character of the type, and exhibiting it, under certain conditions, even in a higher degree, the question naturally suggested itself, What would be the effect of introducing ethyl into phosphoretted, antimonetted, and arsenetted hydrogen?

The ethylation of these hydrogen-compounds presents difficulties not experienced with the nitrogen-series, and has been accomplished only by roundabout processes. Nor have all the terms, the exist-

* Ammonia, ethylamine, diethylamine, and triethylamine placed side by side ; the alkaline properties of these four substances experimentally demonstrated. Iodide of ethyl, and its action on ammonia exhibited.

ence of which theory suggests, as yet been obtained: compounds corresponding to ethylamine and to diethylamine are wanting at present, but the substances which correspond to triethylamine are known.

The following table exhibits the compounds belonging to this group which are known.

Nitrogen-Series.	*Phosphorus-Series.*	*Antimony-Series.*	*Arsenic-Series.*
Ammonia . . . $N\begin{cases}H\\H\\H\end{cases}$	Phosphoretted Hydrogen $P\begin{cases}H\\H\\H\end{cases}$	Antimonetted Hydrogen $Sb\begin{cases}H\\H\\H\end{cases}$	Arsenetted Hydrogen $As\begin{cases}H\\H\\H\end{cases}$
Ethylamine . . $N\begin{cases}E\\H\\H\end{cases}$	Unknown.	Unknown.	Unknown.
Diethylamine . $N\begin{cases}E\\E\\H\end{cases}$	Unknown.	Unknown.	Unknown.
Triethylamine . $N\begin{cases}E\\E\\E\end{cases}$	Triethylphosphine . . . . $P\begin{cases}E\\E\\E\end{cases}$	Triethylstibine $Sb\begin{cases}E\\E\\E\end{cases}$	Triethylarsine $As\begin{cases}E\\E\\E\end{cases}$

Now triethylphosphine,* triethylstibine, and triethylarsine are substances exhibiting, although in a less prominent degree, all the fundamental characters of triethylamine, and consequently of ammonia itself. They are well defined and powerful bases, capable of uniting with the acids and of producing a series of remarkable, mostly crystalline salts, in which we find all the properties of the ammoniacal salts. Chemists have thus succeeded in rendering visible to the mental eye, if I may say so, the true nature of phosphoretted, antimonetted, and arsenetted hydrogen. By the conversion of these *mineral* substances into *organic* compounds, by the simple process of ethylation, their alkaline disposition, not to use the term character, has been unmistakeably brought to light. The formation of alkaline bodies similar to ammonia by the substitution of ethyl for the hydrogen in phosphoretted, antimonetted, and arsenetted hydrogen, leaves no doubt regarding the analogy of these substances with ammonia, and thus we see *that researches carried out exclusively in the field of* ORGANIC CHEMISTRY *have lent most valuable assistance in deciding a question of considerable importance regarding the classification of* MINERAL SUBSTANCES. These researches have furnished the last argument which was wanting to prove that nitrogen, phosphorus, antimony, and arsenic form a natural group of elements, the chemical history of which presents

* Specimens of triethylphosphine exhibited, and its alkaline characters demonstrated. A small quantity of triethylphosphine was poured into a test-tube filled with oxygen, and placed in hot water, when the phosphorus-compound exploded with great violence. Another portion introduced into chlorine gas, gave rise to a brilliant flash of light, the carbon of the substance being set free. To show the combining power of triethylphosphine, the substance was mixed with iodide of methyl, when a white crystalline compound was immediately formed, with evolution of much heat.

analogies not less prominent than those which are observed with the elements chlorine, bromine, and iodine.

The type Ammonia offers another interesting illustration of the influence which the progress of Organic Chemistry exerts upon the Mineral Department of the science, and of the unexpected support which some of the mineral theories have received from the development of our ideas regarding the constitution of organic substances.

Soon after Sir Humphry Davy's immortal discoveries of the alkali-metals, chemists were led by the extraordinary analogy of the salts of these metals with those of ammonia, to assume in the latter a metallic substance similar to potassium and sodium.* Numerous experiments were made to isolate this metallic principle from the ammoniacal salts; and the resources of electricity, which had exhibited such wonderful powers in the hands of Sir H. Davy, were not appealed to without result. The metal itself, it is true, was not isolated; but a compound or alloy was obtained, containing nitrogen and hydrogen, and the metallic character of which was indisputable. If the electric current be passed into a solution of ammonia floating upon a layer of mercury in such a manner, that the positive pole of the battery merely dips into the ammonia, while the negative pole is immersed in the mercury, a very remarkable phenomenon is observed; the mercury begins to swell up and is gradually converted into a mass of buttery consistence, but retaining a perfect *metallic* lustre, while pure nitrogen gas arising from the oxidation of ammonia makes its appearance at the positive pole.† Removed from the influence of the battery, the altered mercury soon resumes its original appearance, losing at the same time hydrogen and ammonia in the proportion of one equivalent of the former (H) to one of the latter ($N\,H_3$). It was therefore argued that the mercury owed the alteration of its properties to its being associated with hydrogen and ammonia, that is with $N\,H_4$; and since mercury in its combinations never retains any metallic appearance, except in its alloys called amalgams, that is in its combination with metallic substances, chemists considered themselves entitled to attribute *metallic* characters also to the hypothetical association of nitrogen and hydrogen, represented by the formula $N\,H_4 = Am$, for which, forthwith, the name of *Ammonium* was proposed.

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* The analogy of the salts of ammonia and those of the fixed alkalies, and especially the isomorphism of the salts of ammonia and potassa, was illustrated by numerous specimens upon the Lecture-table. Ammonia-alum and potassa-alum, sulphate of ammonia and sulphate of potassa, chloride of ammonium and chloride of potassium, &c. &c.

† The ammonium-amalgam was produced on a small scale by the action of the electric current.

By assuming the existence of this hypothetical metal in the salts of ammonia, the nature of these substances, their analogy with the salts of the fixed alkalies, and especially the isomorphism of the salts of ammonia and potassa, became at once intelligible. The following table exhibits the ammoniacal salts (represented firstly, as combinations *of ammonia*, with hydrated acids; and secondly, as *ammonium*-compounds;) in juxtaposition with the corresponding terms of the potassium-series.

	Ammonia-Compounds.	*Ammonium-Compounds.*		*Potassium-Compounds.*
Oxide .	NH_3, HO	$NH_4\ O$ or	$Am\ O$	$K\ O$
Chloride	NH_3, HCl	$NH_4\ Cl$ or	$Am\ Cl$	$K\ Cl$
Sulphate	NH_3, HSO_4	$NH_4\ SO_4$ or	$Am\ SO_4$	$K\ SO_4$
Nitrate .	NH_3, HNO_6	$NH_4\ NO_6$ or	$Am\ NO_6$	$K\ NO_6$

The analogy of chloride, sulphate, and nitrate of ammonium, with the corresponding potassium-compounds, is complete, but the analogy begins to fail when we compare the *oxides* of ammonium and potassium. Oxide of potassium, potash, is a perfectly definite body, the properties of which, especially in its hydrated condition, are well known. On the other hand all attempts to isolate the oxide of ammonium or its hydrate have been hitherto abortive. Liberated from one of the ammonium-compounds it splits at once into ammonia-gas and water, even at the common temperature. The impossibility of producing the oxide of ammonium has been always adduced as an argument against the ammonium-theory.

This difficulty disappears entirely if we examine the deportment of some of the compounds briefly described in the preceding part of this discourse. By again submitting triethylamine, that is ammonia containing three equivalents of ethyl in the place of three of hydrogen, to the action of iodide of ethyl, a beautiful crystalline compound is obtained, the composition of which is represented by the formula—

$$N\left\{\begin{matrix}E\\E\\E\\E\end{matrix}\right\}I$$

that is iodide of ammonium, in which the four hydrogen-equivalents are replaced by a corresponding number of equivalents of ethyl, or *iodide of tetrethylammonium.* If this compound be treated with freshly precipitated oxide of silver, a decomposition takes place,

which gives rise to the formation of iodide of silver, separating as a precipitate and of the hydrated ammonium-oxide, corresponding to the above-mentioned iodide or *hydrated oxide of tetrethylammonium*,* which remains in solution. The following equation elucidates this change—

$$N\left\{\begin{matrix}E\\E\\E\\E\end{matrix}\right\}I + Ag\,O + H\,O = Ag\,I + N\left\{\begin{matrix}E\\E\\E\\E\end{matrix}\right\}O, H\,O$$

Iodide of tetrethylammonium. Hydrated oxide of tetrethylammonium.

The solution of this compound oxide of ammonium may be evaporated to dryness without decomposition; a crystalline substance is thus obtained of a most powerfully alkaline character, resembling in every respect hydrated potassa itself. A concentrated solution of this substance not only burns the tongue, but acts upon the epidermis which it destroys like potassa or soda. On rubbing the solution between the fingers, the well-known soapy sensation produced by the fixed alkalies under the same circumstances is felt. Moreover the same peculiar odour is perceived. Oxide of tetrethylammonium saponifies the fats without difficulty; beautiful soft soaps are thus obtained, possessing all the properties of ordinary potassa-soap. All the chemical effects produced by potassa or soda, are likewise produced by oxide of tetrethylammonium; and in its deportment with the salts of the metals especially, the compound oxide of ammonium can scarcely be distinguished from the fixed caustic alkalies.

The formation of oxide of tetrethylammonium is certainly one of the most powerful props which the ammonium-theory could have received, and since this support has been entirely and exclusively furnished by researches performed within the domain of *Organic Chemistry*, it must be admitted that the researches made in this department of the science, and especially the elaboration of the compound derivatives of the type ammonia, begin to react most powerfully and beneficially upon the progress of *Mineral Chemistry*.†

[A. W. H.]

* In three beakers, boiling solutions of iodide of tetrethylammonium (1), of iodide of ammonium (2), and of iodide of potassium (3), were mixed with freshly precipitated oxide of silver. The beakers were then covered with reddened litmus paper and glass plates; after a few moments the paper covering solution (2), was found to have become intensely blue, in consequence of the volatilized ammonia-gas; whilst the papers covering solutions (1) and (2) did not exhibit the slightest alteration, the liberated oxides of tetrethylammonium and of potassium not being volatile. On *dipping*, however, reddened litmus paper into these two solutions, they were found to be powerfully alkaline.

† At the conclusion of the discourse, the ammonium-amalgam was prepared on a large scale by the action of potassium-amalgam upon a solution of chloride of ammonium.

WEEKLY EVENING MEETING,

Friday, May 23.

SIR BENJAMIN COLLINS BRODIE, Bart. D.C.L. F.R.S.
Vice-President, in the Chair.

F. A. ABEL, ESQ.
DIRECTOR OF THE CHEMICAL ESTABLISHMENT OF THE WAR DEPARTMENT,

On some of the Applications of Chemistry to Military Purposes.

THE numerous and important improvements effected in the general nature, as well as in the methods of manufacture, of the various implements and materials of warfare, since the energies of military men have been recalled into active operation, have arisen in great measure out of the rapid progress made of late years in the application of physical and mechanical sciences to practical purposes. The liberal application of the great facilities now existing for the manufacture of almost every implement of warfare, by ingenious machinery, with great rapidity, and of such perfect uniformity as could not before be attained by the time-consuming efforts of skilled artizans, has been productive of many highly important improvements in military science, of which some opinion may be formed by a very cursory inspection of the military manufactories which have but recently either started into existence, or expanded from comparatively insignificant to most extensive establishments.

The valuable assistance which chemical science has afforded in the perfection of the implements and materials of warfare, is necessarily less apparent at first sight. It was the object of Mr. Abel to exhibit the intimate connection of chemical science with every branch of military manufacture, by examining briefly, from a chemical point of view, the most important materials and products of the Government establishments for the preparation of war-material.

In a sketch of the applications of the principal metals, allusion was first made to the great attention bestowed by practical men to the question of the manufacture of heavy iron ordnance, from the commencement of the late war, but more particularly since the experience recently gained by the use of iron guns, from various sources of manufacture, had demonstrated the impossibility of placing confidence in guns cast of the description of iron, or by the method, employed of late years by the founders of iron ordnance.

The question as to the material best adapted for heavy guns, or as to the best method of preparing or casting the metal, was still undecided. The most interesting and valuable results had, however, already been obtained by Fairbairn, Whitworth, Nasmyth, and other eminent engineers and metallurgists, and by trials already instituted by the Government, with cannon constructed of various descriptions of iron, and on different principles. The serious attention now directed to the subject by scientific men, added to the excellent opportunities that would be afforded by the extensive Government cannon foundries in course of construction, of practically comparing the value of the most important propositions made with reference to the construction of strong and durable ordnance, promised to place England speedily on a footing with those continental states which have for many years devoted especial attention, and with the greatest success, to this important subject.

In adverting to the extensive application of *galvanized iron* to the erection of light and durable structures, required at a brief notice, reference was also made by the speaker to some ingenious metallurgic applications of *zinc* to the production of compound projectiles for rifled cannon. The property possessed by the so-called *zinc-white* (finely divided oxide of zinc), of retaining its pure white colour on prolonged exposure to atmospheric influence, had led to the almost entire substitution of that colour for white lead, in some important manufacturing departments. The *oxy-chloride of lead* had also been found to possess advantages over white lead, particularly with respect to its covering properties (or *body*). Experiments were being made with a view to determine fully its comparative merits.*

Lead was the metal employed in the purest condition for military purposes. Ordinary pig-lead was used for the manufacture of shot (alloyed with a small quantity of arsenic) and of musquet-bullets, the latter being produced by submitting the lead, cast in rods, to *successive* compressing operations until the form of the bullet was attained. For the manufacture of the rifle-bullets on the *Minié* system, the lead was required to possess a great degree of softness, so that it might be susceptible of instantaneous expansion in the barrel of the rifle, by the force of the exploding powder. Refined lead, as obtained by *Pattinson's* recrystallizing process, was the only quality found to possess the requisite softness. This lead was formed into rods of great length, and converted by Mr. Anderson's ingenious compressing machine, into bullets, in one operation, a single die furnishing 13,000 bullets in ten hours.

* The great liability to adulteration of most descriptions of colours, as also of oils, and a variety of other materials employed in the manufacturing establishments of the War Department, has led to the recent introduction of a rigid system of inspection, to which all articles supplied to these manufactories, which are susceptible of chemical examination, are submitted before they are *accepted for* use in the service.

The bullets used in the shrapnel shell, of which Captain Boxer's improved form was exhibited, were made of an alloy of *lead* and *antimony*. At the present prices of the metals, the value of the alloy would be about £34 per ton; an alloy of these metals, obtained direct from an antimonial lead ore, and sufficiently rich in antimony, was imported in considerable quantities from Hungary, and purchased at a price of about £19 per ton. An antimonial alloy was also used for coating a coarse canvas, forming with it a durable covering for roofs of light buildings, and for the manufacture of screw-plugs, which, being fitted into the fuse-openings of shells charged with powder, rendered the transport of the latter in that state a matter of perfect safety.

The ease and certainty with which charges of powder, in cannon, were ignited by the explosion of a mixture of the native *sulphide of antimony* with chlorate of potassa and a little glass, either by friction or a blow, was pointed out, and the mode of applying this mixture experimentally demonstrated. In connection with the various ingenious contrivances used for firing guns and mortars by this means, an outline was given of the method lately introduced by Colonel Eardley-Wilmot, of firing guns heavily charged, for proof, by galvanic agency, whereby this hitherto hazardous operation was placed completely under the control of the person conducting the proof.

Tin was employed for the manufacture of protective capsules for the time-fuses invented by Captain Boxer, and, in admixture with a small quantity of copper, or as a coating upon that metal, for the manufacture of large powder-canisters. The most important application of tin was to the formation of the alloy with copper, known as *gun-metal*. In speaking of this alloy, Mr. Abel pointed out some important difficulties with which the bronze gun-founders had to contend, and which were mainly due, on the one hand, to the volatilization of some of the tin, at the temperature to which the metal had to be raised; and on the other, to a tendency to the separation, in the mass of metal, during the cooling of a casting, of small quantities of a very hard white alloy, which, in assuming a crystalline form, frequently gave rise to small cavities in the casting. The first difficulty was to some extent overcome by the addition of the tin to the melted copper and old gun-metal, in the form of an alloy of one of tin to two of copper (known as *hard metal*), at as brief an interval before the metal was cast as was consistent with the formation of a homogeneous alloy; the second difficulty alluded to was the more formidable one, and had hitherto baffled the efforts of metallurgists to overcome it. A searching investigation into the causes, and the possible means of preventing this partial separation of the metals, was at the present time being carried on.

Allusion was made to other alloys of copper, such as *Stirling's* and *Muntz's* metal, and to the employment of sheet copper in the manufacture of percussion caps. Mr. Abel demonstrated experi-

mentally the advantages possessed by the fulminate of mercury over other fulminates, and over detonating mixtures, as a means of firing small-arms; and the necessity of rendering the decomposition of the fulminate of mercury more gradual, and the resulting heat more intense, by employing in admixture with it chlorate of potassa or saltpetre.* The cost of fulminate of mercury had been considerably reduced by the beneficial measure lately introduced by Government, with reference to spirits of wine; a result of the important suggestion of Professors Graham, Hofmann, and Redwood, to mix the spirits of wine required for the arts and scientific purposes with a small quantity of wood-spirits, insufficient to interfere with its applications, but sufficient to preclude the possibility of its being rendered palatable. Alcohol was employed in large quantities for moistening explosive or highly combustible compositions which were subjected to successive blows, as in the preparation of fuses. The introduction of *methylated spirits* for these purposes had proved a matter of great economy, not only in consequence of its comparatively low price, but also because it was no longer impossible, as formerly, to ensure the exclusive application by the workmen of the alcohol to its legitimate use.

Mr. Abel next directed attention to some points of interest in connection with the manufacture of *gunpowder*. The analysis of a number of different specimens of powder, obtained during the war from various English and continental sources, had shown that no important difference, as regards composition, existed between powders varying very much in their most important properties. A comparison was instituted between the methods of manufacture adopted in England, France, Prussia, and Belgium, and between the general properties of the powders of these countries. It was found that the very powerful pressure to which the powder made at Waltham Abbey was submitted, rendered it superior to any other powder, in its uniformity and its power of resisting the effects of transport, and of exposure to the atmosphere; while, on the other hand, it was inferior to continental powders in its inflammability. In the employment of Waltham Abbey-powder in large charges, a considerable proportion always escaped ignition, while the combustion of the softer continental powder, under the same circumstances, was always, comparatively speaking, perfect. The rapidity with which the grain of a soft powder was destroyed by transport, and the injurious influence of moisture upon it, led to the conclusion that, with respect to durability, the advantages were in favour of the hard pressed powder, but that the softer powder was superior from

* In France the preference was given to nitre, as the vapours resulting from the decomposition of chlorate of potassa were liable to excite a corrosive action on the nipple of the gun. In England, nitre had been abandoned in favour of chlorate of potassa, since percussion caps, the priming of which contained the former salt, had been found, after having been in store for some time, to have become so much corroded as to be unserviceable.

an economic point of view, provided it were required for rapid consumption.

Some preliminary experiments lately instituted by the speaker, with the view to examine into the nature of the decomposition of a variety of powders, had shown that the residue left on the ignition of some of the best prepared specimens (composed of the ingredients in the proportions indicated by theory) contained appreciable quantities of *nitric acid;* and that the amount of *charcoal* remaining after the decomposition of such powders formed, in many instances, an important proportion of the total quantity contained in the powder. These circumstances indicated that the manufacture of powder was still susceptible of some improvements, which should tend to render the mingling of the ingredients more perfect. Some patented modifications of the so-called incorporating process were alluded to, which appeared, from trials made with them, to be steps in the right direction.

Mr. Abel stated that, after an extensive series of experiments, it had been determined to substitute for the tedious process of refining saltpetre, hitherto adopted at Waltham Abbey, by repeated recrystallizations, the more expeditious and economical and equally efficient method in use upon the Continent, which consists in causing the saltpetre to be rapidly deposited in minute crystals from a hot solution, by maintaining the latter in constant motion till cool; in draining thoroughly the so-called *saltpetre-flour*, and in removing the adherent impurities by two or three successive washings, first with the wash-water from former operations, and finally with pure water.

In conclusion, reference was made to the employment of other explosive and inflammable mixtures, differing in their composition more or less widely from gunpowder. These might be classed under three heads: Firstly, those consisting of the ingredients of gunpowder in varied proportions, examples of which were the compositions for fuses and rockets; secondly, those employed as incendiary or smoke-producing agents, in the preparation of which such materials as *resin*, *bituminous coal*, *pitch*, *boiled oil*, *Venice turpentine*, *zinc*, and *antimony*, were employed, in addition to the gunpowder-constituents;* and thirdly, those which contained metals or metallic compounds capable of imparting to flame various tints, and which were employed as signal-lights, or for pyrotechnic compositions.

* It is very generally known that numerous propositions have been submitted to Government, during the war, for the employment of coal-tar naphtha as an incendiary material, and for applying, as agents of destruction, not only some of the most inflammable, but also many of the most poisonous substances known to chemists. None of these have, however, met with application; partly from the difficulties encountered in contriving means for the safe transport and application of the most effective of such agents; and partly from the natural hesitation of military men to have recourse to such novel weapons of destruction.

The most important of these materials for producing coloured fires were stated to be the *oxide* and *sulphide of copper*, and the *chlorate of copper and potassa;* the *nitrate of lead*, the *sulphide of arsenic (orpiment);* the *sulphide of mercury (Ethiops mineral)*, and the *sub-chloride (calomel); zinc, antimony*, and *iron*, as metals, in the state of filings, &c.; the *carbonate, nitrate*, and *chlorate of baryta;* and the *carbonate* and *nitrate of strontia. Chlorate of potassa* was largely employed to increase the vehemence of the combustion of many compositions containing colouring substances, whereby the brilliancy of the resulting tints is much heightened. The chlorate of baryta had lately been prepared on a very large scale for pyrotechnic compositions, and large specimens of this very beautiful salt were exhibited. In endeavouring to prepare a compound of the *chlorate of copper* with *ammonia* (similar to the so-called ammonio-nitrate of copper), as a material for a brilliant purple fire, Mr. Nicholson had obtained a beautifully crystalline compound, of so powerfully explosive a character, that even its syrupy solution detonated sharply when struck with a hammer upon an anvil. The substance in question was more dangerous to manipulate with than fulminate of mercury, but it underwent gradual decomposition on exposure to air, accompanied by a diminution of its explosive properties. Some experiments in connection with this compound had led to the observation that the ammonio-nitrate of copper, when thoroughly dry, also possessed slight detonating properties.

Mr. Abel concluded his discourse by expressing the hope that he had succeeded in laying before his audience some interesting proofs of the intimate connection of chemical with military science, and of the successful practical applications of the results of scientific research to purposes, which have proved so recently, to possess the highest national importance.

[F. A. A.]

WEEKLY EVENING MEETING,

Friday, May 30.

SIR RODERICK I. MURCHISON, G.C.S. F.R.S. Vice-President, in the Chair.

DR. LYON PLAYFAIR, C.B. F.R.S.

On the Chemical Principles involved in Agricultural Experiments.

DR. PLAYFAIR commenced by pointing out the modern views in regard to the food of plants. This may be divided into Air Food and Earth Food. The *air food* contains carbonic acid, water, ammonia, and nitric acid. Humus, to which great value was formerly attached by vegetable physiologists, is now known to act by its decay as an earth-provider of these substances. Although the soil and plants have the power of absorbing ammonia directly from the atmosphere, still the largest portion must be supplied to them in solution, either in the form of rain or dew. Our knowledge on this subject is still imperfect. The average fall of rain on an acre may be taken in this country at 2270 tons. Taking the largest results for the ammonia found in rain water, only 30 lbs. of nitrogen would thus be supplied to crops, while a fair crop of cereals, growing in a few weeks only, contains 50 lbs. of nitrogen; a crop of turnips contains 100 lbs., and one of mangold and clover, 150 lbs. No doubt nitric acid furnishes a considerable quantity of the nitrogen brought down by rain, but probably the dew is the more active agent, and this falls in proportion as diligence applied to the cultivation of land increases its radiating surface.

With regard to the *earth food*, attention was drawn to the essential ingredients in all plants, and to the characteristic quantities of each in crops of different kinds. This was shown by curves, representing the abstracted ingredients of the soil in the usual crops.

Within certain limits the air food may be viewed as of a constant composition and quantity, the diffusion of air equalising it over all districts. But the earth food is constantly varying, both in quality and quantity. Both air food and earth food being necessary conditions of fertility, the sterility or diminished fertility of a field must depend upon that condition of growth which is variable, and not upon that which is constant. The soil (the variable condition)

contains its ingredients either free or imprisoned, that is, either soluble or insoluble. The action of air, rain, and frost, liberates the imprisoned substances, rendering them available to plants. The mechanical operations of the farm, ploughing, harrowing, clod crushing, draining, &c., have this end in view. Jethro Tull ascribed all success to such operations, and we find as long ago as the time of Cato, that the weathering effects on the soil were well known. But sooner or later the fertile ingredients of a soil must be removed by crops; and to prevent sterility we supply, by *manure*, what we take away by cultivation. The *primary* action of manure must be to render to the soil that which is taken away; or in other words, to produce a constant condition of growth in that which would otherwise be variable: but its secondary action often is, like humus, to supply an excess of air food in order to gain time in cultivation. Nutrition of plants must be directly proportional to the quantity of the necessary ingredients, and inversely proportional to the obstacles to their assimilation.

The quantity of ingredients of earth food is constant for the same crop, within certain limits, arising from the greater or less development of particular organic substances in them. It is with plants as with animals. Formerly experiments used to be made with the latter by confining them to certain substances present in food. Dogs were fed separately on starch, or gelatine, or sugar, and they died, because all the conditions of nutrition were not satisfied. So it is with plants. If a single necessary ingredient be omitted, the plant cannot grow. A child could not be expected to thrive, if bone earth were carefully kept out of its food; it might have flesh in abundance, enough to grow a little Hercules, or fat enough for a cherub, but without phosphate of lime it would refuse to grow. Exactly the same law rules vegetable growth. This, expressed as a law of fertility, means THAT THE BODY *in minimo* RULES THE CROP. If, for instance, bone earth be the body present in *least* quantity, and potash, soda, lime, &c. be present in excess, the extent of the crop will depend only upon the amount of bone earth, and the amounts of the other substances taken up will be exactly proportional to the limit of the former. All the bone earth being removed, the excess of the other substances is of no use, for the crop will refuse to grow. Add, however, an excess of bone earth, the crop will grow to the extent of the next substance, *in minimo*, which may be sulphuric acid, or any other necessary ingredient.

After explaining these general laws of fertility, the establishment of which are entirely due to Liebig, Dr. Playfair proceeded to apply them to the recent experiments made by farmers, and which were supposed by them to be irreconcileable with the pervailing notions of agricultural chemistry. He drew attention to the experiments of Mr. Lawes, who, aided by Dr. Gilbert, has carried on conscientiously, and with a full desire to arrive at just conclu-

sions, a series of trials on a scale rather befitting a public body than a private individual. As a general result of these it was found that mineral manures alone did not suffice for full crops of cereals, but that ammonia was required to be added to them for proper success. The air then did not supply enough of the latter substance. As regards root crops, such as turnips, the result was different, mineral bodies, especially phosphates and sulphates, being found to be highly beneficial, while ammonia did not appear to be required as an addition to the manure. Alkalies were found favourable to the leguminous crops. When the different habits of the plants are considered, the results appear to be more comprehensible. Cereals have a small expanse of leaf, and a short period of life. In the 17 weeks of their growth at Rothamstead, they receive 800 or 900 tons of water as rain, of which about 500 tons are evaporated by passing through the crop. But in reality they make half their dry substance in four or five weeks. Even admitting that they received all the ammonia of the rain, only about 12 lbs. of nitrogen would be thus received by the crops, instead of the 50 lbs. which they require. In the case of the short-lived cereals, to which a gain of time is everything, it would be natural to expect that an augmentation of ammonia would be favourable to their growth.

The turnip, on the other hand, grows steadily over 21 weeks, making dry substance all the time, and with its broad leaves can take in more air food. Then, as regards earth food. The wheat has long greedy roots, which it throws out in all directions in search of food ; the turnip, with its small delicate fibres, is dependant on the food in its immediate vicinity. The wheat is an accomplished forager, like the light Zouave, and if food be in the soil it will procure it. The turnip is like the bulky English soldier who, unless food is brought up to his tent door, is likely to fare badly. These habits of the plants determine why an artificial supply of an ingredient of air food is more necessary to one, and of earth food to the other; but this result, though a valuable accession to our knowledge, in no way shakes the original laws of nutrition.

It is not, however, quite clear that even cereals with high cultivation may not get ammonia enough for themselves out of the air, without an artificial supply being given. At Lois Weedon, a soil frequently stirred and well worked has, without any manure, grown for ten years full crops of wheat of 34 bushels, on half an acre placed under growth, the other half being kept under fallow. In this instance, the absorption and radiating power of the soil being much increased by the frequent stirring, more ammonia is absorbed, and more dew containing ammonia is deposited, while the weathering of the soil has hitherto liberated sufficient mineral ingredients for full crops.

If no other conclusion had been drawn by farmers from the Rothamstead experiments than that, in soils of an ordinary condition, an artificial supply of ammonia must be furnished to cereals, a

practical result of importance would have been arrived at; but they have laid down as an agricultural dogma, that "nitrogen is the manure for wheat, and phosphorus and sulphates for turnip," thus re-introducing the notion of *specifics* into the laws of manure. What, in the present stage of physiology, would be thought of a similar dogma in regard to animals? Because a horse contains more muscle, though less fat than a cow, it would not be permitted as a law of nutrition, to say that "Carbon is the food for a cow, and nitrogen for a horse;" or because the sinewy Arab contains less bone earth than the large-boned Highlander, "Nitrogen is the food for an Arab, and phosphorus for a Highlander."

This introduction of the idea of *specifics* into agriculture is dropping the veil of Isis over the whole subject. The sum of nutrition is made up of two factors, air food and earth food. Both factors are of equal importance. To discuss whether air food or earth food does most for particular crops is like discussing which side of a pair of scissors is most useful in cutting, or whether the upper or lower jaw is of most use in chewing. Dr. Playfair discussed at length the conditions of durable fertility of a soil, showing that the earth food was the capital of the farmer, and that any diminution in his capital should only be made by a deliberate and intelligent decision. For example, on a limestone soil it would be legitimate to draw upon lime without replacement, or in heavy clay soils upon alkalies. But as no soil is equally rich in all ingredients, an unintelligent draught on the soil will soon destroy it, for when *one* ingredient of earth food becomes *in minimo* the fertility is reduced to its proportion, and is destroyed when it is used up.

Dr. Playfair then proceeded to show how the chief recent experiments in manures, which were rendered graphically in diagrams, bore out these views. Among others a series of experiments made in Saxony to show the duration of the action of manures led to conclusive results. Thus, in one case, an addition of 11 lbs. of phosphoric acid produced an augmentation of a half more crop of clover containing 158 lbs. *more* of earth food and nitrogen, thus showing, not that phosphoric acid was a specific, but that it was the body *in minimo*, and this being supplied, the crop was enabled to thrive and appropriate from the air and the soil the large quantities of other ingredients necessary for its development, but which were of no use when one ingredient was deficient in its necessary proportional quantity.

All the variations of district or local agriculture, instead of representing specifics, which varying in them, would by contradictory experience destroy one another, represent only immediate requirements of particular soils having different bodies *in minimo*.

The only "specific" that should be admitted into farming is a knowledge of the laws upon which nutrition depends. As long as farming is carried on without an acquaintance with these laws on the part of its cultivators, great progress cannot be expected,

and uncertain counsels will always prevail. It is not the duty of such philosophers as Liebig to make the direct applications of the laws of nature which they discover to the actual practice of the field. It is, however, the duty of the practical man thoroughly to understand these laws, and to find their technical applications for himself, for this is his *art*, as the other is the *science* of the philosopher. The recent review of the agricultural experiments which are supposed to be so antagonistic to Liebig's views of the science, Dr. Playfair had endeavoured to show were, when properly discussed, strongly confirmatory of them, and the antagonism was due not to any inherent contradiction between the philosopher and the farmer, but to a want of understanding as to their relative positions and duties to each other.

[L. P.]

GENERAL MONTHLY MEETING,

Monday, June 2.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

James C. C. Bell, Esq. Consul for the Grand Duchy of Tuscany.
Charles Oliver Frederick Cator, Esq.
William Crawford, Esq.
Frederick William Irby, Esq.
William Gibbs, Esq.
John Smith, Esq. and
Theodore Talbot, Esq.

were duly *elected* Members of the Royal Institution.

George Hudswell Westerman, Esq.

was *admitted* a Member of the Royal Institution.

The following Presents were announced, and the thanks of the Members returned for the same:—

From

Airey, G. B. Esq. F.R.S. Astronomer-Royal—Discussion of the Deviations of the Compass in Wood-built and Iron-built Ships. (Phil. Trans.) 4to. 1856.

Allies, T. W. Esq. M.A. M.R.I. (the Author)—The See of St. Peter, the Rock of the Church, the Source of Jurisdiction, and the Centre of Unity. 16mo. 1855.
Astronomical Society, Royal—Monthly Notices. Vol. XVI. No. 7. 8vo. 1856.
Memoirs, Vol. XXIV. 4to. 1856.
Monthly Notices, Vol. XV. 8vo. 1854-5.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for June, 1856. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for May, 1856. 4to.
Bradbury, Henry, Esq. M.R.I. (the Author)—On the Security and Manufacture of Bank-Notes; a Lecture. 4to. 1856.
The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Part 14. fol. 1856.
British Architects, Royal Institute of—Proceedings in May 1856. 4to.
British Museum, Trustees of the—List of Birds. Part 4. 12mo. 1856.
List of Lepidopterous Insects. Part 7. 12mo. 1856.
Civil Engineers, Institute of—Proceedings in May 1856. 8vo.
Commissioners for the Exhibition of 1851—Third Report. 8vo. 1856.
Domville, Sir W., Bart. M.R.I. (the Author)—The Sabbath: or an Inquiry into the Supposed Obligation of the Sabbaths of the Old Testament. 2 vols. 8vo. 1849-55.
Dublin Society, Royal—Journal, Vol. I. Part 1. 8vo. 1856.
East India Company, the Hon.—Magnetical and Meteorological Observations at Bombay in 1852. 4to. 1855.
Editors—The Medical Circular for May 1856. 8vo.
The Practical Mechanic's Journal for May 1856. 4to.
The Journal of Gas-Lighting for May 1856. 4to.
The Mechanic's Magazine for May 1856. 8vo.
The Athenæum for May 1856. 4to.
The Engineer for May 1856. fol.
The Literarium for May 1856. fol.
Faraday, Professor, D.C.L. F.R.S. (the Author)—Experimental Researches in Electricity. Thirtieth Series. (Phil. Trans.) 4to. 1856.
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WEEKLY EVENING MEETING,

Friday, June 6.

Sir Roderick I. Murchison, G.C.S. F.R.S. Vice-President, in the Chair.

John Tyndall, Esq. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY IN THE ROYAL INSTITUTION.

Comparative View of the Cleavage of Crystals and Slate Rocks.

When the student of physical science has to investigate the character of any natural force, his first care must be to purify it from the mixture of other forces, and thus study its simple action. If, for example, he wishes to know how a mass of water would shape itself, supposing it to be at liberty to follow the bent of its own molecular forces, he must see that these forces have free and undisturbed exercise. We might perhaps refer him to the dew-drop for a solution of the question; but here we have to do, not only with the action of the molecules of the liquid upon each other, but also with the action of gravity upon the mass, which pulls the drop downwards and elongates it. If he would examine the problem in its purity, he must do as Plateau has done, withdraw the liquid mass from the action of gravity, and he would then find the shape of the mass to be perfectly spherical. Natural processes come to us in a mixed manner, and to the uninstructed mind are a mass of unintelligible confusion. Suppose half-a-dozen of the best musical performers to be placed in the same room, each playing his own instrument to perfection: though each individual instrument migh be a wellspring of melody, still the mixture of all would produce mere noise. Thus it is with the processes of nature. In nature mechanical and molecular laws mingle and create apparent confusion. Their mixture constitutes what may be called the *noise* of natural laws; and it is the vocation of the man of science to resolve this noise into its components, and thus to detect the "music" in which the foundations of nature are laid.

The necessity of this detachment of one force from all other

forces is nowhere more strikingly exhibited than in the phænomena of crystallization. I have here a solution of sulphate of soda. Prolonging the mental vision beyond the boundaries of sense, we see the atoms of that liquid, like squadrons under the eye of an experienced general, arranging themselves into battalions, gathering round a central standard, and forming themselves into solid masses, which after a time assume the visible shape of the crystal which I here hold in my hand. I may, like an ignorant meddler wishing to hasten matters, introduce confusion into this order. I do so by plunging this glass rod into the vessel; the consequent action is not the pure expression of the crystalline forces; the atoms rush together with the confusion of an unorganized mob, and not with the steady accuracy of a disciplined host. Here also in this mass of bismuth we have an example of this confused crystallization; but in the crucible behind me a slower process is going on: here there is an architect at work "who makes no chips, no din," and who is now building the particles into crystals, similar in shape and structure to those beautiful masses which we see upon the table. By permitting alum to crystallize in this slow way, we obtain these perfect octahedrons; by allowing carbonate of lime to crystallize, nature produces these beautiful rhomboids; when silica crystallizes, we have formed these hexagonal prisms capped at the ends by pyramids; by allowing saltpetre to crystallize we have these prismatic masses, and when carbon crystallizes, we have the diamond. If we wish to obtain a perfect crystal, we must allow the molecular forces free play: if the crystallizing mass be permitted to rest upon a surface it will be flattened, and to prevent this a small crystal must be so suspended as to be surrounded on all sides by the liquid, or, if it rest upon the surface, it must be turned daily so as to present all its faces in succession to the working builder. In this way the scientific man nurses these children of his intellect, watches over them with a care worthy of imitation, keeps all influences away which might possibly invade the strict morality of crystalline laws, and finally sees them developed into forms of symmetry and beauty which richly reward the care bestowed upon them.*

In building up crystals, these little atomic bricks often arrange themselves into layers which are perfectly parallel to each other, and which can be separated by mechanical means; this is called the cleavage of the crystal. I have here a crystallized mass which has thus far escaped the abrading and disintegrating forces which sooner or later determine the fate of sugar-candy. If I am skilful enough I shall discover that this crystal of sugar cleaves

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* To Mr. Pattinson, of the Felling Chemical Works, Newcastle-upon-Tyne, I am indebted for some fine specimens of crystallized alum and carbonate of soda.

with peculiar facility in one direction. Here again I have a mass of rock-salt: I lay my knife upon it, and with a blow cleave it in this direction; but I find on further examining this substance that it cleaves in more directions than one. Laying my knife at right angles to its former position, the crystal cleaves again; and finally placing the knife at right angles to the two former positions, the mass cleaves again. Thus rock-salt cleaves in three directions, and the resulting solid is this perfect cube, which may be broken up into any number of smaller cubes. Here is a mass of Iceland spar, which also cleaves in three directions, not at right angles, but oblique to each other, the resulting solid being a rhomboid. In each of these cases the mass cleaves with equal facility in all three directions. For the sake of completeness I may say that many substances cleave with unequal facility in different directions, and the heavy spar I hold in my hand presents an example of this kind of cleavage.

Turn we now to the consideration of some other phænomena to which the term cleavage may be applied. This piece of beech-wood cleaves with facility parallel to the fibre, and if our experiments were fine enough we should discover that the cleavage is most perfect when the edge of the axe is laid across the rings which mark the growth of the tree. The fibres of the wood lie side by side, and a comparatively small force is sufficient to separate them. If you look at this mass of hay severed from a rick, you will see a sort of cleavage developed in it also; the stalks lie in parallel planes, and only a small force is required to separate them laterally. But we cannot regard the cleavage of the tree as the same in character as the cleavage of the hayrick. In the one case it is the atoms arranging themselves according to organic laws which produce a cleavable structure; in the other case the easy separation in a certain direction is due to the mechanical arrangement of the coarse sensible masses of the stalks of hay.

In like manner I find that this piece of sandstone cleaves parallel to the planes of bedding. This rock was once a powder, more or less coarse, held in mechanical suspension by water. The powder was composed of two distinct parts, fine grains of sand and small plates of mica. Imagine a wide strand covered by a tide which holds such powder in suspension:* how will it sink? The rounded grains of sand will reach the bottom first, the mica afterwards, and when the tide recedes, we have the little plates shining like spangles upon the surface of the sand. Each successive tide brings its charge of mixed powder, deposits its duplex layer day after day, and finally masses of immense thick-

* I merely use this as an illustration; the deposition may have really been due to sediment carried down by rivers. But the action must have been periodic, and the powder duplex.

ness are thus piled up, which by preserving the alternations of sand and mica tell the tale of their formation. I do not wish you to accept this without proof. Take the sand and mica, mix them together in water, and allow them to subside, they will arrange themselves in the manner I have indicated; and by repeating the process you can actually build up a sandstone mass which shall be the exact counterpart of that presented by nature, as I have done in this glass jar. Now this structure cleaves with readiness along the planes in which the particles of mica are strewn. Here is a mass of such a rock sent to me from Halifax: here are other masses from the quarries of Over Darwen, in Lancashire.* With a hammer and chisel you see I can cleave them into flags; indeed these flags are made use of for roofing purposes in the districts from which the specimens have come, and receive the name of "slatestone." But you will discern without a word from me, that this cleavage is not a crystalline cleavage any more than that of a hayrick is. It is not an arrangement produced by molecular forces; indeed it would be just as reasonable to suppose that on this jar of sand and mica the particles arranged themselves into layers by the forces of crystallization, instead of by the simple force of gravity, as to imagine that such a cleavage as this could be the product of crystallization.

This, so far as I am aware of, has never been imagined; and it has been agreed among geologists not to call such splitting as this cleavage at all, but to restrict the term to a class of phænomena which I shall now proceed to consider.

Those who have visited the slate quarries of Cumberland and North Wales will have witnessed the phænomena to which I refer. We have long drawn our supply of roofing-slates from such quarries; schoolboys ciphered on these slates, they were used for tombstones in churchyards, and for billiard-tables in the metropolis; but not until a comparatively late period did men begin to inquire how their wonderful structure was produced. What is the agency which enables us to split Honister Crag, or the cliffs of Snowdon, into laminæ from crown to base? This question is at the present moment one of the greatest difficulties of geologists, and occupies their attention perhaps more than any other. You may wonder at this. Looking into the quarry of Penrhyn, you may be disposed to explain the question, as I heard it explained two years ago. "These planes of cleavage," said a friend who stood beside me on the quarry's edge, "are the planes of stratification which have been lifted by some convulsion into an almost vertical position." But this was a great mistake, and indeed here lies the grand difficulty of the problem. These planes of cleavage stand in most cases at a high angle to the bedding. Thanks to Sir Roderick

* For the specimens from Halifax I have to thank Mr. Richard Carter, and for those from Darwen I am indebted to Mr. J. Singleton.

Murchison, who has kindly permitted me the use of specimens from the Museum of Practical Geology (and here I may be permitted to express my acknowledgments to the distinguished staff of that noble establishment, who, instead of considering me an intruder, have welcomed me as a brother), I am able to place the proof of this before you. Here is a mass of slate in which the planes of bedding are distinctly marked; here are the planes of cleavage, and you see that one of them makes a large angle with the other. The cleavage of slates is therefore not a question of stratification, and the problem which we have now to consider is, "By what cause has this cleavage been produced?"

In an able and elaborate essay on this subject in 1835, Professor Sedgwick proposed the theory that cleavage is produced by the action of crystalline or polar forces after the mass has been consolidated. "We may affirm," he says, "that no retreat of the parts, no contraction of dimensions in passing to a solid state, can explain such phænomena. They appear to me only resolvable on the supposition that crystalline or polar forces acted upon the whole mass simultaneously in one direction and with adequate force." And again, in another place: "Crystalline forces have rearranged whole mountain masses, producing a beautiful crystalline cleavage, passing alike through all the strata."* The utterance of such a man struck deep, as was natural, into the minds of geologists, and at the present day there are few who do not entertain this view either in whole or in part.† The magnificence of the theory, indeed, has, in some cases, caused speculation to run riot, and we have books published, aye and largely sold, on the action of polar forces and geologic magnetism, which rather astonish those who know something about the subject. According to the theory referred to, miles and miles of the districts of North Wales and Cumberland, comprising huge mountain masses, are neither more nor less than the parts of a gigantic crystal. These masses of slate were originally fine mud; this mud is composed of the broken and abraded particles of older rocks. It contains silica, alumina, iron, potash, soda, and mica mixed in sensible masses mechanically together. In the course of ages the mass became consolidated, and the theory before us assumes that afterwards a process of crystallization rearranged the particles and developed in

* Transactions of the Geological Society, ser. ii. vol. iii. p. 477.

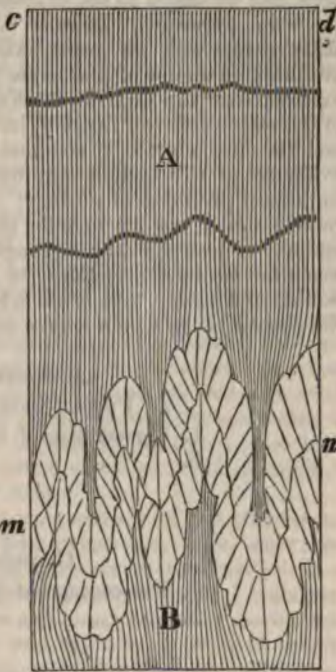
† In a letter from Sir Charles Lyell, dated from the Cape of Good Hope, February 20, 1836, Sir John Herschel writes as follows:—"If rocks have been so heated as to allow of a commencement of crystallization, that is to say, if they have been heated to a point at which the particles can begin to move amongst themselves, or at least on their own axes, some general law must then determine the position in which these particles will rest on cooling. Probably that position will have some relation to the direction in which the heat escapes. Now when all or a majority of particles of the same nature have a general tendency to one position, that must of course determine a cleavage plane."

the mass a single plane of crystalline cleavage. With reference to this hypothesis, I will only say that it is a bold stretch of analogies: but still it has done good service; it has drawn attention to the question; right or wrong a theory thus thoughtfully uttered has its value; it is a dynamic power which operates against intellectual stagnation; and even by provoking opposition is eventually of service to the cause of truth. It would however have been remarkable if, among the ranks of geologists themselves, men were not found to seek an explanation of the phænomena in question, which involved a less hardy spring on the part of the speculative faculty than the view to which I have just referred.

The first step in an inquiry of this kind is to put oneself into contact with nature, to seek facts. This has been done, and the labours of Sharpe (the late President of the Geological Society, who, to the loss of science and the sorrow of all who knew him, has so suddenly been taken away from us), Sorby, and others, have furnished us with a body of evidence which reveals to us certain important physical phænomena, associated with the appearance of slaty cleavage, if they have not produced it: the nature of this evidence we will now proceed to consider.

Fossil shells are found in these slate-rocks. I have here several specimens of such shells, occupying various positions with regard to the cleavage planes. They are squeezed, distorted, and crushed. In some cases a flattening of the convex shell occurs, in others the valves are pressed by a force which acted in the plane of their junction; but in all cases the distortion is such as leads to the inference that the rock which contains these shells has been subjected to enormous pressure in a direction at right angles to the planes of cleavage; the shells are all flattened and spread out upon these planes. I hold in my hand a fossil trilobite of normal proportions. Here is a series of fossils of the same creature which have suffered distortion. Some have lain across, some along, and some oblique to the cleavage of the slate in which they are found; in all cases the nature of the distortion is such as required for its production a compressing force acting at right angles to the planes of cleavage. As the creatures lay in the mud in the manner indicated, the jaws of a gigantic vice appear to have closed upon them and squeezed them into the shape you see. As further evidence of the exertion of pressure, let me introduce to your notice a case of contortion which has been adduced by Mr. Sorby. The bedding of the rock shown in this figure was once horizontal; at A we have a deep layer of mud, and at *m n* a layer of comparatively unyielding gritty material; below that again, at B, we have another layer of the fine mud of which slates are formed. This mass cleaves along the shading lines of the diagram: but look at the shape of the intermediate bed: it is contorted into a serpentine form, and leads irresistibly to the conclusion that the *mass has* been pressed together at right angles to the planes of

cleavage. This action can be experimentally imitated, and I have here a piece of clay in which this is done and the same result produced on a small scale.



The amount of compression, indeed, might be roughly estimated by supposing this contorted bed *m n* to be stretched out, its length measured and compared with the distance *c d;* we find in this way that the yielding of the mass has been considerable.

Let me now direct your attention to another proof of pressure; you see the varying colours which indicate the bedding on this mass of slate. The dark portion, as I have stated, is gritty, and composed of comparatively coarse particles, which, owing to their size, shape and gravity, sink first and constitute the bottom of each layer. Gradually, from bottom to top the coarseness diminishes, and near the upper surface of each layer we have a mass of comparatively fine clean mud. Sometimes this fine mud forms distinct layers in a mass of slate-rock, and it is the mud thus consolidated from which are derived the German razor-stones, so much prized for the sharpening of surgical instruments. I have here an example of such a stone; when a bed is thin, the clean white mud is permitted to rest, as in this case, upon a slab of the coarser slate in contact with it: when the bed is thick, it is cut into slices which are cemented to pieces of ordinary slate, and thus rendered stronger. The mud thus deposited sometimes in layers is, as might be expected, often rolled up into nodular masses, carried forward, and deposited by the rivers from which the slate-mud has subsided. Here, indeed, are such nodules enclosed in sand-stone. Everybody who has ciphered upon a school-slate must remember the whitish-green spots which sometimes dotted the surface of the slate; he will remember how his slate-pencil usually slid over such spots as if they were greasy; now these spots are composed of the finer mud, and they could not, on account of their fineness, *bite* the pencil like the surrounding gritty portions of the slate. Here is a beautiful example of the spots: you observe them on the cleavage surface in broad patches; but if this mass has been compressed at right angles to the planes

even if these plates of mica were wholly absent the cleavage of slate rocks would be much the same as it is at present.

I will not dwell here upon minor facts,—I will not urge that the perfection of the cleavage bears no relation to the quantity of mica present; but I will come at once to a case which to my mind completely upsets the notion that such plates are a necessary element in the production of cleavage.

Here is a mass of pure white wax: there are no mica particles here; there are no scales of iron, or anything analogous mixed up with the mass. Here is the self-same substance submitted to pressure. I would invite the attention of the eminent geologists whom I see before me to the structure of this mass. No slate ever exhibited so clean a cleavage; it splits into laminæ of surpassing tenuity, and proves at a single stroke that pressure is sufficient to produce cleavage, and that this cleavage is independent of the intermixed plates of mica assumed in Mr. Sorby's theory. I have purposely mixed this wax with elongated particles, and am unable to say at the present moment that the cleavage is sensibly affected by their presence,—if anything, I should say they rather impair its fineness and clearness than promote it.

The finer the slate the more perfect will be the resemblance of its cleavage to that of the wax. Compare the surface of the wax with the surface of this slate from Borrodale in Cumberland. You have precisely the same features in both: you see flakes clinging to the surfaces of each, which have been partially torn away by the cleavage of the mass: I entertain the conviction that if any close observer compares these two effects, he will be led to the conclusion that they are the product of a common cause.*

But you will ask me how, according to my view, does pressure produce this remarkable result? This may be stated in a very few words.

Nature is everywhere imperfect! The eye is not perfectly achromatic, the colours of the rose and tulip are not pure colours, and the freshest air of our hills has a bit of poison in it. In like manner there is no such thing in nature as a body of perfectly homogeneous structure. I break this clay which seems so intimately mixed, and find that the fracture presents to my eyes innumerable surfaces along which it has given way, and it has yielded along these surfaces because in them the cohesion of the mass is less than elsewhere. I break this marble, and even this wax, and observe the same result: look at the mud at the bottom of a dried pond; look to some of the ungravelled walks in Kensington Gar-

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* I have usually softened the wax by warming it, kneaded it with the fingers, and pressed it between thick plates of glass previously wetted. At the ordinary summer temperature the wax is soft, and tears rather than cleaves; on this account I cool my compressed specimens in a mixture of pounded ice and salt, and when thus cooled they split beautifully.

dens on drying after rain,—they are cracked and split, and other circumstances being equal, they crack and split where the cohesion of the mass is least. Take then a mass of partially consolidated mud. Assuredly such a mass is divided and subdivided by surfaces along which the cohesion is comparatively small. Penetrate the mass, and you will see it composed of numberless irregular nodules bounded by surfaces of weak cohesion. Figure to your mind's eye such a mass subjected to pressure,—the mass yields and spreads out in the direction of least resistance;* the little nodules become converted into laminæ, separated from each other by surfaces of weak cohesion, and the infallible result will be that such a mass will cleave at right angles to the line in which the pressure is exerted.

Further, a mass of dried mud is full of cavities and fissures. If you break dried pipe-clay you see them in great numbers, and there are multitudes of them so small that you cannot see them. I have here a piece of glass in which a bubble was enclosed; by the compression of the glass the bubble is flattened, and the sides of the bubble approach each other so closely as to exhibit the colours of thin plates. A similar flattening of the cavities must take place in squeezed mud, and this must materially facilitate the cleavage of the mass in the direction already indicated.

Although the time at my disposal has not permitted me to develope this thought as far as I could wish, yet for the last twelve months the subject has presented itself to me almost daily under one aspect or another. I have never eaten a biscuit during this period in which an intellectual joy has not been superadded to the more sensual pleasure; for I have remarked in all such cases cleavage developed in the mass by the rolling-pin of the pastrycook or confectioner. I have only to break these cakes, and to look at the fracture, to see the laminated structure of the mass. Nay, I have the means of pushing the analogy further; I have here some slate which was subjected to a high temperature during the conflagration of Mr. Scott Russell's premises. I invite you to compare this structure with that of a biscuit: air or vapour within the mass has caused it to swell, and the mechanical structure it reveals is precisely that of a biscuit. I have gone a little into the mysteries of baking while conducting my inquiries on this subject, and have received much instruction from a lady friend in the manufacture of puff-paste. Here is some such paste baked in this house under my own superintendence. The cleavage of our hills is accidental cleavage, but this is cleavage with intention. The volition of the pastrycook has entered into the formation of the mass, and it has been his aim to preserve a series of surfaces of structural weakness,

* It is scarcely necessary to say that if the mass were squeezed equally in *all* directions no laminated structure could be produced; it must have room to yield in a lateral direction.

along which the dough divides into layers. Puff-paste must not be handled too much, for then the continuity of the surfaces is broken; it ought to be rolled on a cold slab, to prevent the butter from melting, and diffusing itself through the mass, thus rendering it more homogeneous and less liable to split. This is the whole philosophy of puff-paste; it is a grossly exaggerated case of slaty cleavage.*

As time passed on cases multiplied, illustrating the influence of pressure in producing lamination. Mr. Warren De la Rue informs me that he once wished to obtain white-lead in a fine granular state, and to accomplish this he first compressed the mass: the mould was conical, and permitted the mass to spread a little laterally under the pressure. The lamination was as perfect as that of slate, and quite defeated him in his effort to obtain a granular powder. Mr. Brodie, as you are aware, has recently discovered a new kind of graphite: here is the substance in powder, of exquisite fineness. This powder has the peculiarity of clinging together in little confederacies; it cannot be shaken asunder like lycopodium; and when the mass is squeezed, these groups of particles flatten, and a perfect cleavage is produced. Mr. Brodie himself has been kind enough to furnish me with specimens for this evening's discourse. I will cleave them before you: you see they split up into plates which are perpendicular to the line in which the pressure was exerted. This testimony is all the more valuable, as the facts were obtained without any reference whatever to the question of cleavage.

I have here a mass of that singular substance Boghead cannel.† This was once a mass of mud, more or less resembling this one, which I have obtained from a bog in Lancashire. I feel some hesitation in bringing this substance before you, for, as in other cases, so in regard to Boghead cannel, science—not science, let me not libel it, but the quibbling, litigious, money-loving portion of human nature speaking through the mask of science—has so contrived to split hairs as to render the qualities of the substance somewhat mythical. I shall therefore content myself with showing you how it cleaves, and with expressing my conviction that pressure had a great share in the production of this cleavage.

The principle which I have enunciated is so simple as to be almost trivial; nevertheless, it embraces not only the cases I have mentioned: but, if time permitted, I think I could show you that it takes a much wider range. When iron is taken from the puddling

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* Cream cheese, at least such as I have tried, when torn asunder by the fingers, shows a very perfect cleavage in planes perpendicular to the direction in which the mass has been squeezed. In an ordinary loaf of household bread, the portion near the under crust may also be torn into laminæ: this is perhaps best seen when the bread is fresh.

† For which I have to thank Mr. George Edmondson.

furnace it is a more or less spongy mass: it is at a welding heat, and at this temperature is submitted to the process of rolling: bright smooth bars such as this are the result of this rolling. But I have said that the mass is more or less spongy or nodular, and, notwithstanding the high heat, these nodules do not perfectly incorporate with their neighbours: what then? You would say that the process of rolling must draw the nodules into fibres—it does so; and here is a mass acted upon by dilute sulphuric acid, which exhibits in a striking manner this fibrous structure. The experiment was made by my friend Dr. Percy, without any reference to the question of cleavage.

Here are other cases of fibrous iron. This fibrous structure is the result of mechanical treatment. Break a mass of ordinary iron and you have a granular fracture; beat the mass, you elongate these granules, and finally render the mass fibrous. Here are pieces of rails along which the wheels of locomotives have slided;* the granules have yielded and become plates. They exfoliate or come off in leaves; all these effects belong, I believe, to the great class of phænomena of which slaty cleavage forms the most prominent example.†

Thus, ladies and gentlemen, we have reached the termination of our task. I commenced by exhibiting to you some of the phænomena of crystallization. I have placed before you the facts which are found to be associated with the cleavage of slate rocks. These facts, as finely expressed by Helmholtz, are so many telescopes to our spiritual vision, by which we can see backward through the night of antiquity, and discern the forces which have been in operation upon the earth's surface

"Ere the lion roared,
Or the eagle soared."

From evidence of the most independent and trustworthy character, we come to the conclusion that these slaty masses have been subjected to enormous pressure, and by the sure method of experiment we have shown—and this is the only really new point which has been brought before you—how the pressure is sufficient to produce the cleavage. Expanding our field of view, we find the self-same law, whose footsteps we trace amid the crags of Wales and Cumberland, stretching its ubiquitous fingers into the domain of the pastrycook and ironfounder; nay, a wheel cannot roll over

* For these specimens and other valuable assistance I am indebted to Mr. Williams.

† An eminent authority informs me that he believes these surfaces of weak cohesion to be due to the interposition of films of graphite, and not to any tendency of the iron itself to become fibrous; this of course does not in any way militate against the theory which I have ventured to propose. All that the theory requires is surfaces of weak cohesion, however produced, and a change of shape of such surfaces consequent on pressure or rolling.

the half-dried mud of our streets without revealing to us more or less of the features of this law. I would say, in conclusion, that the spirit in which this problem has been attacked by geologists, indicates the dawning of a new day for their science. The great intellects who have laboured at geology, and who have raised it to its present pitch of grandeur, were compelled to deal with the subject in mass; they had no time to look after details. But the desire for more exact knowledge is increasing; facts are flowing in, which, while they leave untouched the intrinsic wonders of geology, are gradually supplanting by solid truths the uncertain speculations which beset the subject in its infancy. Geologists now aim to imitate, as far as possible, the conditions of nature, and to produce her results; they are approaching more and more to the domain of physics; and I trust the day will soon come when we shall interlace our friendly arms across the common boundary of our sciences, and pursue our respective tasks in a spirit of mutual helpfulness, encouragement, and goodwill.

[J. T.]

WEEKLY EVENING MEETING,

Friday, June 13.

SIR RODERICK I. MURCHISON, G.C.S. F.R.S. Vice-President, in the Chair.

PROFESSOR FARADAY, D.C.L. F.R.S.

On M. Petitjean's process for Silvering Glass: some Observations on divided Gold.

M. PETITJEAN's process consists essentially in the preparation of a solution containing oxide of silver, ammonia, nitric, and tartaric acids, able to deposit metallic silver either at common or somewhat elevated temperatures; and in the right application of this solution to glass, either in the form of plates or vessels. 1540 grains of nitrate of silver being treated with 955 grains of strong solution of ammonia, and afterwards with 7700 grains of water, yields a solution, to which when clear 170 grains of tartaric acid dissolved in 680 grains of water is to be added, and then 152 cubic inches more of water, with good agitation. When the liquid has settled, *the clear part* is to be poured off; 152 cubic inches of water to be

added to the remaining solid matter, that as much may be dissolved as possible; and the clear fluids to be put together and increased by the further addition of 61 cubic inches of water. This is the silvering solution, No. 1; a second fluid, No. 2, is to be prepared in like manner, with this difference, that the tartaric acid is to be doubled in quantity. The apparatus employed for the silvering of glass plate consists of a cast-iron table box, containing water within, and a set of gas-burners beneath to heat it: the upper surface of the table is planed and set truly horizontal by a level, and covered by a varnished cloth: heat is applied until the temperature is 140° Fah. The glass is well cleaned, first with a cloth; after which a plug of cotton, dipped in the silvering fluid and a little polishing powder, is carefully passed over the surface to be silvered, and when this application is dry it is removed by another plug of cotton, and the plate obtained perfectly clean. The glass is then laid on the table, a portion of the silvering fluid poured on to the surface, and this spread carefully over every part by a cylinder of india-rubber stretched upon wood which has previously been cleaned and wetted with the solution; in this manner a perfect wetting of the surface is obtained, and all air bubbles, &c. are removed. Then more fluid is poured on to the glass until it is covered with a layer about the $\frac{1}{10}$th of an inch in depth, which easily stands upon it; and in that state its temperature is allowed to rise. In about 10 minutes or more silver begins to deposit on the glass, and in 15 or 20 minutes a uniform opaque coat, having a greyish tint on the upper surface, is deposited. After a certain time the glass employed in the illustration was pushed to the edge of the table, was tilted that the fluid might be poured off, was washed with water, and then was examined. The under surface presented a perfectly brilliant metallic plate of high reflective power, as high as any that silver can attain to; and the coat of silver, though thin, was so strong as to sustain handling, and so firm as to bear polishing on the back to any degree, by rubbing with the hand and polishing powder. The usual course in practice, however, is, when the first stratum of fluid is exhausted, to remove it, and apply a layer of No. 2 solution; and when that has been removed and the glass washed and dried, to cover the back surface with a protective coat of black varnish. When the form of the glass varies, simple expedients are employed; and by their means either concave or convex, or corrugated surfaces are silvered, and bottles and vases are coated internally. It is easy to mend an injury in the silvering of a plate, and two or three cases of repair were performed on the table.

The proposed advantages of the process are,—the production of a perfect reflecting surface; the ability to repair; the mercantile economy of the process (the silver in a square yard of surface is worth 1*s.* 8*d.*); the certainty, simplicity, and quickness of the operation; and, above all, the dismissal of the use of mercury. In

theory the principles of the process justify the expectations, and in practice nothing as yet has occurred which is counter to them.

With regard to the second part of the evening's discourse, the speaker said he had been led by certain considerations to seek experimentally for some effect on the rays of light, by bodies which when in small quantities had strong peculiar action upon it, and which also could be divided into plates and particles so thin and minute as to come far within the dimensions of an undulation of light, whilst they still retained more or less of the power they had in mass; and though he had as yet obtained but little new information, he considered it his duty, in some degree, to report progress to the Members of the Royal Institution. The vibrations of light are, for the violet ray 59,570 in an inch, and for the red ray 37,640 in an inch; it is the lateral portion of the vibration of the ether* which is by hypothesis supposed to affect the eye, but the relation of number remains the same. Now a leaf of gold as supplied by the mechanician is only $\frac{1}{280,000}$ of an inch in thickness, so that $7\frac{1}{2}$ of these leaves might be placed in the space occupied by a single undulation of the red ray, and 5 in the space occupied by a violet undulation. Gold of this thickness and in this state is transparent, transmitting green light, whilst yellow light is reflected; there is every reason to believe also that some is absorbed, as happens with all ordinary bodies. When gold leaf is laid upon a layer of water on glass, the water may easily be removed, and solutions be substituted for it; in this way a solution of chlorine, or of cyanide of potassium, may be employed to thin the film of gold; and as the latter dissolves the other metals present in the gold, (silver, for instance, which chlorine leaves as a chloride,) it gives a pure result; and by washing away the cyanide, and draining and drying the last remains of water, the film is left attached to the glass: it may be experimented with, though in a state of extreme tenuity. Examined either by the electric lamp, or the solar spectrum, or the microscope, this film was apparently continuous in many parts where its thickness could not be a tenth or twentieth part of the original gold leaf. In these parts gold appeared as a very transparent thing, reflecting yellow light and transmitting green and other rays; it was so thin that it probably did not occupy more than a hundredth part of a vibration of light, and yet there was no peculiar effect produced. The rays of the spectrum were in succession sent through it; a part of all of them was either stopped or turned back, but that which passed through was *unchanged* in its character, whether

* Analogous transverse vibration may easily be obtained on the surface of water or other fluids, by the process described in the Philosophical Transactions for 1831, p. 336, &c.

the gold plate was under ordinary circumstances, or in a very intense magnetic field of force.

When a solution of gold is placed in an atmosphere containing phosphorus vapour the gold is reduced, forming films that may be washed and placed on glass without destroying their state or condition: these vary from extreme thinness to the thickness of gold leaf or more, and have various degrees of reflective and transmissive power; they are of great variety of colour, from grey to green, but they are like the gold leaves in that they do not change the rays of light.

When gold wires are deflagrated by the Leyden discharge upon glass plates, extreme division into particles is effected, and deposits are produced, appearing, by transmitted light, of many varieties of colour, amongst which are ruby, violet, purple, green, and grey tints. By heat many of these are changed so as to transmit chiefly ruby tints, retaining always the reflective character of gold. None of them affect any particular ray selected from the solar spectrum, so as to change its character, otherwise than by reflection and absorption; what is transmitted still remains the same ray. When gold leaf is heated on glass the heat causes its retraction and running together. To common observation the gold leaf disappears, and but little light is then reflected or stopped: but if pressure by a polished agate convex surface be applied to the gold in such places, reflective power reappears to a greater or smaller degree, and green light is again transmitted. When the gold films by phosphorus have been properly heated, pressure has the same effect with them.

If a piece of clean phosphorus be placed *beneath* a weak gold solution, and especially if the phosphorus be a clear thick film, obtained by the evaporation of a solution of that substance in sulphide of carbon, in the course of a few hours the solution becomes coloured of a ruby tint; and the effect goes on increasing, sometimes for two or three days. At times the liquid appears clear, at other times turbid. As far as Mr. Faraday has proceeded, he believes this fluid to be a mixture of a colourless transparent liquid, with fine particles of gold. By transmitted light, it is of a fine ruby tint; by reflected light, it has more or less of a brown yellow colour. That it is merely a diffusion of fine particles is shown by two results; the first is, that the fluid being left long enough the particles settle to the bottom: the second is, that whilst it is coloured or turbid, if a cone of the sun's rays (or that from a lamp or candle in a dark room) be thrown across the fluid by a lens, the particles are illuminated, reflect yellow light, and become visible, not as independent particles, but as a cloud. Sometimes a liquid which has deposited much of its gold, remains of a faint ruby tint, and to the ordinary observation, transparent; but when illuminated by a cone of rays the suspended particles show their presence by the opalescence, which is the result of their united action. The settling particles, if in a flask, appear

at the bottom, like a lens of deep coloured fluid, opaque at the middle, but deep ruby at the edges; when agitated they may be again diffused through the liquid. These particles tend to aggregate into larger particles, and produce other effects of colour. It is found that boiling gives a certain degree of permanence to the ruby state. Many saline and other substances affect this ruby fluid: thus, a few drops of solution of common salt being added, the whole gradually becomes of a violet colour; still the particles are only in suspension, and when illuminated by a lens are a golden yellow by reflected light: they separate now much more rapidly and perfectly by deposition from the fluid than before. Some specimens, however, of the fluid, of a weak purple or violet colour, remain for months without any appearance of settling, so that the particles must be exceedingly divided; still the rays of the sun or even of a candle in a dark room, when collected by a lens, will manifest their presence. The highest powers of the microscope have not as yet rendered visible either the ruby or the violet particles in any of these fluids.

Glass is occasionally coloured of a ruby tint by gold; such glass, when examined by a ray of light and a lens, gives the opalescent effect described above, which indicates the existence of separate particles; at least such has been the case with all the specimens Mr. Faraday has examined. It becomes a question whether the constitution of the glass and the ruby fluids described is not, as regards colour, alike. At present, he believes they are; but whether the gold is in the state of pure metal, or of a compound, he has yet to decide. It would be a point of considerable optical importance if they should prove to be metallic gold; from the effects presented when gold wires are deflagrated by the Leyden discharge over glass, quartz, mica, and vellum, and the deposits subjected to heat, pressure, &c., he inclines to believe they are pure metal.

[M. F.]

GENERAL MONTHLY MEETING,

Monday, July 7.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

The Right Hon. Edward Cardwell, M.P.
Frederick Ducane Godman, Esq. and
George Knight Erskine Fairholme, Esq.

were duly *elected* Members of the Royal Institution.

Frederick Wm. Irby, Esq.

was *admitted* a Member of the Royal Institution.

The following PRESENTS were announced, and the thanks of the Members returned for the same :—

FROM

Acland, Thomas Dyke, Esq.—A Word in Season: or, How to grow Wheat with Profit. 15th Edition. 8vo. 1856.
Ashmolean Society, Oxford—Transactions, Vols. I. and II. 8vo. 1835-52.
Proceedings, Nos. 1-3. 8vo. 1832-55.
Report on the Mortality and Public Health of Oxford, in 1849-50. 8vo. 1854.
Asiatic Society of Bengal—Journal, No. 252. 8vo. 1855.
Asiatic Society, Royal—Journal, Vol. XVI. Part 2. 8vo. 1856.
Astronomical Society, Royal—Monthly Notices. Vol. XVI. No. 7. 8vo. 1856.
Author—Remarks on the Decimal System of Coinage. 8vo. 1856.
Bath and West of England Society—Journal, Parts 1, 2, and 4. 8vo. 1853-6.
Bayley, Francis, Esq. M.R.I.—Fifty-one Miscellaneous Tracts, 1648-1721. 4to.
Fifty Sermons, Charges, &c. 1678-1794. 4to.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for July, 1856. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for June, 1856. 4to.
Bombay Geographical Society—Transactions, Vol. XII. 8vo. 1856.
Bombay Medical Board—Deaths in Bombay in 1854.—Correspondence respecting Burials in Bombay. 4to. 1855.
Bradbury, Henry, Esq. M.R.I.—The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Part 15. fol. 1856.
Brewer, Thomas, Esq. (the Author)—Memoir of John Carpenter, Town-Clerk of London in the reigns of Henry V. and Henry VI. 8vo. 1856.
British Architects, Royal Institute of—Proceedings in June, 1856. 4to.
Carpenter, W. B. M.D. F.R.S. (the Author)—Researches on the Foramenifera. (Phil. Trans.) 4to. 1856.
Crichton, Alexander, Esq. M.R.I.—Œuvres de M. De Pradt. 24 vols. 8vo. Paris, 1817-28.
Commissioners in Lunacy—Ninth Report. 8vo. 1856.
Dublin Geological Society—Journal, Vols. I.-VI. 8vo. 1834-55.
East India Company, the Hon.—Magnetical and Meteorological Observations at Bombay in 1853. 4to. 1855.

Editors—The Medical Circular for June 1856. 8vo.
The Practical Mechanic's Journal for June 1856. 4to.
The Journal of Gas-Lighting for June 1856. 4to.
The Mechanic's Magazine for June 1856. 8vo.
The Athenæum for June 1856. 4to.
The Engineer for June 1856. fol.
The Literarium for June 1856. fol.
Falconer, H. M.D. (the Author)—On Prof. Huxley's Attempted Refutation of Cuvier. 8vo. 1856.
Faraday, Professor, D.C.L. F.R.S.—Monatsberichte der Konigl. Preuss. Akademie, Mai, 1856. 8vo. Berlin.
Memorie della Reale Accademia della Scienze di Torino. Serie Seconda. Tomo XV. 4to. 1855.
Almanach fur 1855. 16mo.
Feste-Rede und Denkrede. 4to. 1855.
Annalen der Königlichen Sternwarte bei München: von Dr. J. Lamont. Band VII. and VIII. 8vo. 1854-5.
Bayerische Akademie—Abhandlungen der Math. Phys. Classe. Band VII. Abth. 3. 4to. 1855.
Genève, Société de Physique—Mémoires. Tome XIV. Partie 1. 4to. 1855.
Geographical Society, Royal—Proceedings, No. 3. 8vo. 1856.
Journal, Vol. XXV. 8vo. 1855.
Graham, George, Esq. Registrar-General—Report of the Registrar-General for June 1856. 8vo.
Jennings, Richard, Esq. M.A. M.R.I. (the Author)—Social Delusions concerning Wealth and Want. 16mo. 1856.
Knight, Mr. G. (the Publisher)—Tracts on Photographic Manipulation, by J. How, G. Le Gray, and T. Hennah. 12mo. 1855.
Law, W. J. Esq. A.M. (the Author)—Reply to Mr. Ellis's Defence of his Theory on the Route of Hannibal. 8vo. 1856.
Macloughlin, D. M.D. M.R.I. (the Author)—On Cholera. 8vo. 1856.
Macrory, E. Esq. M.R.I.—Lucani Pharsalia cum J. Sulpitii Verulani et Omniboni Vicentini Commentariis. fol. [Ven. 1493.]
Mendicity Society—Reports. 8vo. 1846-55.
Newton, Messrs.—London Journal (New Series), July 1856. 8vo.
Novello, Mr. (the Publisher)—The Musical Times for June 1856. 4to.
Northumberland, the Duke of, K.G. F.R.S. President R.I.—Descriptive Catalogue of a Cabinet of Roman Family Coins, belonging to the Duke of Northumberland, K.G. by Rear-Admiral W. H. Smyth. 4to. 1856.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1856. Heft 3, 4, 5. 4to. Gotha, 1856.
Photographic Society—Journal, No. 43. 8vo. 1856.
Royal Society of London—Proceedings, No. 21. 8vo. 1856.
Report of the Committee of Physics and Meteorology. 8vo. 1840.
Catalogue of the Library of the Royal Society. 4to. 1825.
Catalogues of Miscellaneous Literature and MSS. 2 vols. 8vo. 1840-1.
Astronomical Observations, by Rev. T. Catton. 4to. 1853.
Sharp, Hercules, Esq. M.R.I.—Archivio Centrale di Stato in Firenze. 8vo. 1855.
Society of Arts—Journal for June 1856. 8vo.
Statistical Society—Journal, Vol. XIX. Part 2. 8vo. 1856.
Tennant, Professor, and Rev. W. Mitchell (the Authors)—Mineralogy and Crystallography. 12mo. 1856.
Vereins zur Beförderung des Gewerbfleisses in Preussen—März und April 1856. 4to.
Vincent, B. Assist.-Sec. R.I.—The Anglo-Saxon Church. By H. Soames. 8vo. 1835.

Royal Institution of Great Britain.

GENERAL MONTHLY MEETING,

Monday, November 3.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Sir Charles Henry Rouse Boughton, Bart.
George Burdon, Esq. and
William Frederick Robinson, Esq.

were duly *elected* Members of the Royal Institution.

The special thanks of the Members were returned to the Right Hon. SIR BENJAMIN HALL, Bart. M.P. for his present of a copy of "Architectural Antiquities of the Collegiate Church of St. Stephen's Chapel, Westminster. By F. Mackenzie." fol. 1844.

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM—

Actuaries, Institute of—Assurance Magazine. Nos. 24, 25. 8vo. 1856.
Agricultural Society, Royal—Journal, Vol. XVII. Part 1. 8vo. 1856.
Asiatic Society of Bengal—Journal, Nos. 253–255. 8vo. 1855.
Astronomical Society, Royal—Monthly Notices. Vol. XVI. No. 8. 8vo. 1856.
Barclay, A. W. M.D. M.R.I. (the Author)—The Progress of Preventive Medicine and Sanitary Measures. 8vo. 1856.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for Aug. to Nov. 1856. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for July to Oct. 1856. 4to.
Bradbury, Henry, Esq. M.R.I.—The Ferns of Great Britain and Ireland. By T. Moore, F.L.S. Edited by J. Lindley, Ph.D. F.L.S. Parts 16, 17. fol. 1856.
British Architects, Royal Institute of—Proceedings in Session 1855–6. 4to.
British Association—Report of the Meeting at Glasgow in 1855. 8vo. 1856.
British Museum, Trustees of the—Catalogues of the Natural History Collections. 8 Parts. 12mo. 1856.
Brockhurst, Rev. Joseph S. (the Author)—The Wife: or Love and Madness. A Tragedy. 8vo. 1856.
Brodhurst, Bernard E. Esq. M.R.I. (the Author)—On the Nature and Treatment of Club Foot. 8vo. 1856.
Chemical Society—Journal, Nos. 34, 35. 8vo. 1856.

Coutts, Miss Angela Burdett, M.R.I.—Summary Account of Prizes for Common Things, offered and awarded by Miss Burdett Coutts, at the Whitelands Training Institution, 1855-6. 8vo. 1856.
Crichton, Alex., Esq. M.R.I.—Comte J. Potocki, Histoire Ancienne des Provinces de l'Empire de Russie. 3 Parties. 4to. St. Pétersbourg, 1804-5.
Dynasties du Second Livre de Manéthon. 12mo. Florence. 1803.
Chronologie de Manéthon. 4to. St. Petersbourg, 1805.
Principes de Chronologie pour les temps antérieurs aux Olympiades. 4to. St. Petersbourg, 1810.
De la Rue, Warren, Esq. F.R.S. M.R.I.—Engraving of Saturn, as seen with a Newtonian Equatoreal of 13 inches aperture. March 27, 28. 1856.
Dilke, C. Wentworth, Esq. M.R.I.—Publications of the Ælfric Society. 15 Parts. 1843-56.
Traité Elémentaire de Geométrie Descriptive. Par H. Tresca. 8vo. 1852.
Historical Account of the Origin and Progress of Astronomy. By John Narrien. 8vo. 1833.
Dublin Geological Society—Journal, Vol. VII. Parts 1, 2. 8vo. 1856.
Dublin Society, Royal—Journal, Nos. 2, 3. 8vo. 1856.
Editors—The Medical Circular for July to October, 1856. 8vo.
The Practical Mechanic's Journal for July to October, 1856. 4to.
The Journal of Gas-Lighting for July to October, 1856. 4to.
The Mechanic's Magazine for July to October, 1856. 8vo.
The Athenæum for July to October, 1856. 4to.
The Engineer for July to October, 1856. fol.
The Literarium for July to October, 1856.
St. James's Medley. No. 7. 8vo. 1856.
Ellis, Alex. J. Esq. (the Author)—Universal Writing and Printing for the Use of Missionaries, Linguists, &c. 4to. 1856.
Faraday, Professor, D.C.L. F.R.S.—Monatsberichte der Königl. Preuss. Akademie, Juni. 1856. 8vo. Berlin.
Bulletin de la Société Vaudoise. 8vo. Lausanne, 1854-6.
Oversigt over det Kongelige Danske Videnskabernes Selskabs Forhandlinger. 8vo. Kjöbenhavn, 1855.
Abhandlungen der Königlichen Akademie zu Berlin. Erster Supplement Band. fol. 1856.
Kaiserliche Akademie, Wien:—Almanach für 1856. 16mo.
Mathematisch-Naturwissenschaftliche Classe:—Denkschriften, Band X. XI. 4to. 1855-6.
Sitzungsberichte. Band XVIII. XIX. und XX. Heft 1. 8vo. 1855-6.
Jahrbücher der K. K. Central Anstalt für Meteorologie und Erdmagnetismus. Band IV. Jahrgang 1852. Wien, 1856.
Franklin Institute of Pennsylvania—Journal, Vol. XXXI. No. 6. 8vo. 1856.
Geographical Society, Royal—Proceedings, Nos. 4, 5. 8vo. 1856.
Geological Society—Journal, No. 47. 8vo. 1856.
Geological and Polytechnic Society of the West Riding of Yorkshire—Proceedings, 1842-55. 8vo.
Graham, George, Esq. (Registrar-General)—Report of the Registrar-General for July to October, 1856. 8vo.
Hall, Rt. Hon. Sir Benjamin, Bart. First Commissioner of H.M.'s Works—Architectural Antiquities of the Collegiate Chapel of St. Stephen's, Westminster, the late House of Commons. By F. Mackenzie. fol. 1844.
Hopkins, Evan, Esq. (the Author)—On the Vertical Structure of Primary Rocks. 8vo. 1856.
Ingall, G. H., Esq. M.R.I.—Campanalogia improved. 18mo. 1733.
Leeds Philosophical Society—Annual Reports for 1854-6. 8vo.
Linnean Society—Journal, Vol. I. No. 3. 8vo. 1856.
Londesborough, The Lord, K.H. M.R.I.—Miscellanea Graphica, No. 10. 4to. 1856.
London Library—Supplement to Catalogue, Vol. II. 8vo. 1856.

Marcet, W., M.D. (the Author) Life-Sub. R.I.—On the Composition of Food, and how it is Adulterated. 8vo. 1856.
Morgan, Octavius, Esq. M.D. M.R.I. (the Author)—Excavations within Caerwent in 1855. 4to. 1856.
Newton, Messrs.—London Journal (New Series), Aug. to Nov. 1856. 8vo.
Novello, Mr. (the Publisher)—The Musical Times, for July to Oct. 1856. 4to.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie, 1856: Hefte 6, 7, 8. 4to. Gotha, 1856.
Philological Society—Journal, 1856. Part 1. 8vo. 1856.
Photographic Society—Journal, Nos. 44 to 47. 8vo. 1856.
Prosser, John, Esq. Life Sub. R.I.—The Complete Angler. By Is. Walton and C. Cotton. With Lives of the Authors, and Notes by Sir John Hawkins. 7th Edition. With Etchings, by Howitt. S. Bagster. 4to. 1808.
Rennie, James, Esq. F.R.S. M.R.I.—J. J. Berzelius—Théorie des Proportions Chimiques et Table des Poids, &c. 2e Ed. 8vo. Paris, 1835.
Jahres-Bericht über die Fortschritte der Physischen Wissenschaften, übersetzt von F. Wohler. 8vo, Tübingen, 1836, 1837, 1839–41.
Rapport Annuel sur le Progrès de la Chimie, &c. 8vo. 1842-8.
Royal Academy of Sciences, Amsterdam—Verhandelingen, Derde Deel. 4to. 1856.
Verslagen: Natuurkunde, 3e Deel, Stuk 3; 4e Deel; 5e Deel, Stuk 1. Letterkunde, 1e Deel; 2e Deel, Stuk 1. 8vo. 1855–6.
Lycidas, Ecloga, et Musæ Invocatio, Carmina. 8vo. 1856.
Royal Irish Academy—Transactions, Vol. XXIII. Part 1. 4to. 1856.
Proceedings, Vol. VI. Part 3. 8vo. 1855–6.
Royal Observatory, Greenwich—Observations in 1854. 4to. 1856.
Royal Society of Edinburgh—Transactions, Vol. XXI. Part 3. 4to. 1856.
Proceedings, No. 46. 8vo. 1855–6.
Royal Society of London—Proceedings. No. 22. 8vo. 1856.
St. Petersburg Imperial Academy of Sciences—Bulletin de la Classe Physico-Mathématique. Tome XIV. 4to. 1855–6.
Salenave, Dr. L. (the Author)—Maladies Chroniques dues à l'Epuisement. 8vo. Paris, 1855.
Smith, Mr. J. Russell—T. Halbertsma, Prosopographiæ Aristophaneæ. 8vo. 1855.
Smith, W. H., Esq. (the Author)—"Was Lord Bacon the Anthor of Shakspeare's Plays?" 8vo. 1856.
Society of Arts—Journal for July to October 1856. 8vo.
Statistical Society—Journal, Vol. XIX. Part 3. 8vo. 1856.
Staunton, Sir George T., D.C.L. M.P. F.R.S. (the Author)—Memoirs of his Public Life. (For Private Circulation.) 8vo. 1856.
University College, London—Calendar for 1856–7. 12mo. 1856.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Mai zu Juni 1856. 4to.
Zoological Society—Proceedings, Nos. 299–309. 8vo. 1855-6.

GENERAL MONTHLY MEETING,

Monday, December 1.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Charles Freshfield, Esq.
James Morgan, Esq. and
James Plaisted Wilde, Esq. M.A. Q.C.

were duly *elected* Members of the Royal Institution.

The Secretary reported that the following Arrangements had been made for the Lectures before Easter, 1857 :—

Six Lectures on ATTRACTION (adapted to a Juvenile Auditory), by MICHAEL FARADAY, Esq. D.C.L. F.R.S. &c. Fullerian Professor of Chemistry, R.I.

Twelve Lectures on PHYSIOLOGY AND COMPARATIVE ANATOMY —viz. Eight Lectures on SENSATION AND MOTION, and Four Lectures on THE PRINCIPLES OF NATURAL HISTORY, by THOMAS HENRY HUXLEY, Esq. F.R.S. Fullerian Professor of Physiology, R.I.

Eleven Lectures on SOUND, by JOHN TYNDALL, Esq. F.R.S. Professor of Natural Philosophy, R.I.

Ten Lectures on LEADING QUESTIONS IN GEOLOGY, by JOHN PHILLIPS, Esq. F.R.S.

The following PRESENTS were announced, and the thanks of the Members returned for the same :—

FROM—
Acland, H. M.D. F.R.S. (the Author)—Memoir of the Cholera at Oxford in 1854. 4to. 1856.
American Academy of Arts and Sciences—Memoirs. New Series. Vol. V. Part 2. 4to. 1855.
D. Treadwell, on Constructing Cannon. 8vo. 1856.
American Philosophical Society—Proceedings, Vol. VI. Nos. 52, 53. 8vo. 1855.
Asiatic Society of Bengal—Journal, No. 256. 8vo. 1855.
Astronomical Society, Royal—Monthly Notices., Vol. XVI. No. 9. 8vo. 1856.
Bache, Prof. A. D. (the Superintendant)—Report of the United States Coast Survey, for 1853. 4to. 1854.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for Dec. 1856. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for Nov. 1856. 4to.
Boston Natural History Society, U.S.—Proceedings, Vol. V. No. 12-21. 8vo. 1854-5.
British Architects, Royal Institute of—Proceedings in Nov. 1856. 4to.

Collier, Charles, M.D. F.R.S. (the Author)—An Essay on the Principles of Education Physiologically considered. 16to. 1856.
Cornwall Polytechnic Society—Reports, 1854–5. 8vo.
Denison, Edmund Beckett, Esq. Q.C. M.R.I. (the Author)—Lectures on Church-Building, with some Practical Remarks on Bells and Clocks. 2nd Ed. 16to. 1856.
Dublin Geological Society—Journal, Vol. VII. Part 3. 8vo. 1856.
Editors—The Medical Circular for November 1856. 8vo.
The Practical Mechanic's Journal for November 1856. 4to.
The Journal of Gas-Lighting for November 1856. 4to.
The Mechanic's Magazine for November 1856. 8vo.
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1857.

WEEKLY EVENING MEETING,

Friday, January 23.

Sir Charles Fellows, Vice-President, in the Chair.

Professor John Tyndall, F.R.S.

Observations on Glaciers.

The speaker commenced, by stating the circumstances in which the discourse of the evening originated. On the 6th of June, 1856, he had given a lecture upon slaty cleavage, at which Mr. Huxley was present. A short time afterwards his attention was drawn to the observations of Professor J. D. Forbes on the veined structure of glacial ice, by Mr. Huxley, who surmised that the theory of slate cleavage might also apply to the ice structure. On consulting the observations referred to, the probability of the surmise was immediately perceived, and an arrangement was made for a joint visit to the glaciers of Grindelwald, the Aar, and the Rhone. This arrangement was carried out, and the subject being a physical one, it fell to the lot of the speaker to follow it up after his return to England. By reading, reflection, and experiment, the boundaries of the investigation were extended, until finally it embraced the main divisions of the problem of glacier structure and motion. The results of the enquiry constituted the subject of the evening's discourse.

Certain phenomena connected with the motion of glaciers were first passed under review. The power of a glacier to accommodate itself to the sinuosities of its bed, the motion of the mass through a valley of variable width, and a number of similar facts had been adduced as evidence of the ductility of glaciers by M. Rendu and others. To these evidences Professor J. D. Forbes added, in 1842, the important observation that the centre of a glacier moved more quickly than its sides; and he was led finally to a definite expression of his views in the well-known Viscous Theory of glacial motion. Numerous appearances, indeed, seem to favour this idea of viscosity. The aspect of many glaciers, as a whole,—their power of closing up crevasses, and of reconstructing themselves after having been precipitated down glacial gorges,—the bending and

contortions of the ice, the quicker movement of the centre where the ice is uninfluenced by the retardation of the banks, are all circumstances which have been urged with such constancy and ability, as to leave the viscous theory without any formidable competitor at present. To these may also be added, the support which the theory derived from its apparent competency to explain the laminar structure of the ice—a structure regarded by eminent authorities as a crucial test in favour of the viscous theory, and which was affirmed to be impossible of explanation on any other hypothesis.

Nevertheless, this theory is so directly opposed to our ordinary experience of the nature of ice, as to leave a lingering doubt of its truth upon the mind. To remove this doubt, it is urged, that the true nature of ice is to be inferred from experiments upon large masses, and that such experiments place the viscosity of ice in the position of a fact, rather than in that of a theory. It has never been imagined that the bendings and contortions, and other evidences of apparent viscosity exhibited by glaciers, could be made manifest on hand specimens of ice. But this was shown by the speaker to be experimentally possible. Moulds of various forms were hollowed out in boxwood, and pieces of ice were placed in these moulds and subjected to pressure. In this way spheres of ice were flattened into cakes, and cakes formed into transparent lenses. A straight bar of ice, six inches long, was passed through a series of moulds augmenting in curvature, and was finally placed before the audience, bent into a semi-ring. A small block of ice was placed in a hemispherical cavity, and was pressed upon by a hemispherical protuberance not large enough to fill the cavity; the ice yielded and filled the space between both, thus forming itself into a transparent cup. In short, it was shown that every observation made upon glaciers, and adduced by writers on the subject in proof of the viscosity of ice, is capable of perfect imitation with hand specimens of the substance.

These experiments then demonstrate a capacity on the part of small masses of ice, which has hitherto been denied to them. They prove, to all appearance, that the substance is even much more plastic than it was ever imagined to be by the founders of the viscous theory. But the real germ from which these results have sprung, was to be found in a lecture given at the Royal Institution, in June 1850, and reported in the *Athenæum* and *Literary Gazette* for that year. Mr. Faraday then showed, that when two pieces of ice, at a temperature of 32° Fahr., are placed in contact with each other, they freeze together, by the conversion of the film of moisture between them into ice. The case of a snowball was referred to as a familiar illustration of the principle. When the snow is below 32°, and therefore *dry*, it will not cohere, whereas, when it is in a thawing condition, it can be squeezed into a hard mass. During one of the hottest days of last July, when the thermometer was upwards of 100° Fahr. in the sun, and more than 80° in the shade,

the speaker observed a number of blocks of ice, which had been placed loosely in a heap, frozen together at their places of contact; and he afterwards caused them to freeze together under water as hot as the hand could bear. Facts like these suggested the thought, that if a piece of ice—a straight prism, for example—were placed in a bent mould and subjected to pressure it would break, but that the force would also bring its ruptured surfaces into contact, and thus the continuity of the mass might be re-established. Experiment, as we have seen, completely confirmed this surmise: the ice passed from a continuous straight bar to a continuous bent one, the transition being effected, not by a viscous movement of the particles, but *through fracture and regelation.*

All the phenomena of motion, on which the idea of viscosity has been based, are brought by such experiments as the above into harmony with the demonstrable properties of ice. In virtue of this property, the glacier accommodates itself to its bed while preserving its general continuity, crevasses are closed up, and the broken ice of a cascade, such as that of the Talèfre, or the Rhone, is recompacted to a solid continuous mass. But if the glacier accomplish its movement in virtue of the incessant fracture and regelation of its parts, such a process will be accompanied by a crackling noise, corresponding in intensity to the nature of the motion, and which would be absent if the motion were that of a viscous body. It is well known that such noises are heard, from the rudest crashing and quaking, up to the lowest decrepitation, and they thus receive a satisfactory explanation.*

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* It is manifest that the continuity of the fractured ice cannot be completely and immediately restored after rupture. It is not the *same* surfaces that are regelated, and hence the coincidence of the surfaces cannot be perfect. They will enclose for a time *capillary fissures*, and thus the above theory accounts satisfactorily for the known structure of glacier ice. I have recently made the following experiments bearing upon this point. A piece of ordinary ice was taken, and a cavity hollowed in it was filled with a strong infusion of cochineal; the ice was perfectly impervious to the liquid, which remained in it for half an hour without penetrating it in the slightest degree. A piece of the same ice was subjected to a gradually increasing pressure. Flashes of light were seen to issue from it at intervals, indicating the rupture of optical continuity, while a low, and in some instances, almost musical crackling was, at the same time heard. Relieved from the pressure, the ice remained continuous; but a cavity being formed and the infusion placed within it, the coloured liquid immediately diffused itself through the capillary fissures, producing an appearance accurately resembling the drawings illustrative of the infiltration experiments of M. Agassiz upon the glacier of the Aar. This fissured structure, which is inconsistent with the idea of viscosity, is thus shown to be the natural result of the pressures exerted upon the non-viscous and brittle mass of the glacier.

To account for a "*bruit de crépitation*" heard upon the Aar glacier, M. Agassiz refers to an observation which might be made on a fine day in summer, and which would show the air within the glacier ice escaping from its surface. M. Agassiz supposes the ice to be diathermanous; that the sunbeams therefore get through it and heat the air bubbles it encloses, which

The veined or laminar structure of glacier ice was next considered, which Professor J. D. Forbes in his earlier writings compared to slaty cleavage. His theory of the structure is, perhaps, the only one which has made any profound impression, and it may be briefly stated as follows:—Owing to the quicker flow of the centre of a glacier, a sliding of the particles of ice past each other takes place; in consequence of this sliding, fissures are produced, which, when filled with water, and frozen in winter, produce the blue veins of the glacier. To account for the obliquity of the veins to the sides of the glacier, a drag towards the centre is supposed to take place, producing a differential motion in this direction, which results in the formation of fissures. But *at* the centre of the glacier this drag towards it cannot be supposed to exist; and to account for the veins, or laminated structure of the centre, which under normal conditions is transverse to the axis of the glacier, it is supposed that the thrust from behind, meeting an enormous resistance in front produces a differential motion of the particles in a direction approximating to the vertical; and that in consequence of this motion fissures are produced, which, when filled and frozen, produce, as in the other cases, the blue veins. Now, the only *fact* here is that of differential motion *parallel to the length* of the glacier. It is not established that the colds of winter reach to a depth sufficient to produce the blue veins, which it is affirmed form a part of "the inmost structure" of a glacier. Again, the lamination in some cases presents itself in the form of transparent lenticular masses imbedded in the general white ice; and the differential motion referred to would be mechanically inadequate to produce detached cavities corresponding to these masses, which vary greatly in size, and in some cases accurately resemble the greenish spots in slate rock, when a section perpendicular to the cleavage of the rock is exposed. Further, as the motion of the glacier takes place both in summer and winter, it is to be inferred that the fissures are formed at both seasons of the year. If formed in winter, they cannot be filled and frozen that season for want of water; and if formed in summer they cannot, while summer continues, be frozen, for want of cold. Hence, at the end of each summer, if the above theory be correct,

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by their expansion rupture the ice, and produce the crepitation referred to. The observation is an interesting one, whatever difficulty we may find in accepting the explanation. An experiment made on the 31st of January, appears to me to account for the observation in a satisfactory manner. Snow having fallen, I was early at work compressing it; and on removing a plate of the compacted mass from the press, I noticed, as the ice melted, a sparkling motion of the surface. To imitate the action of the sun, an iron spatula was heated, and on bringing it near to the compressed snow, the jumping of the surface caused by the issue of the air through the film of water which covered it, was greatly augmented. On removing the spatula, the motion subsided. To a similar action on the part of the sun, which melts the surface of the glacier, and thus liberates the air bubbles with which it is filled, the observation of M. Agassiz is in all probability to be referred.

there ought to be a whole year's unfrozen fissures in the ice. Such fissures surely would not require the act of freezing to render them visible; they would be seen when filled with blue water just as well as when filled with blue ice. But they never have been observed; and it is therefore to be inferred that they have no existence. With regard to the drag towards the centre, which is supposed to arise from the viscosity, and in which direction it is stated that "filaments slide past each other," it is by no means clear on mechanical grounds that such a drag exists. For the transfer of matter from the sides to the centre, in consequence of such a drag, must finally absorb the former, unless to make good the loss a motion in some other portion of the glacier from the centre to the sides, that is in a direction opposed to the theory, be established. Let the line A B, (Fig. 1) represent the centre of a glacier; C D its side, and *a* a point between both. Draw *m n*, *o p*, making

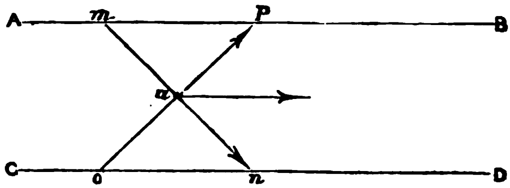
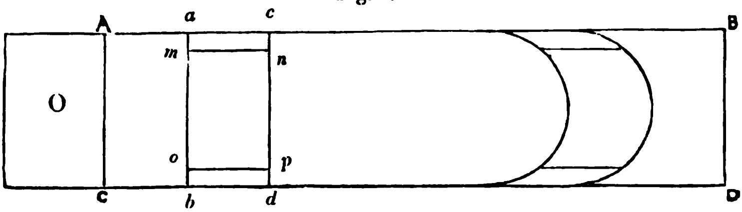


Fig 1.

equal angles with the centre and side. In consequence of the quicker flow of the centre the line *m n* tends to shorten itself, causing a *thrust* on the point *a*, which urges it towards the side C D; the line *o p* for the same reason tends to elongate itself, which produces a *drag* of the point *a* towards the centre. The point *a* is here solicited by two equal forces, and the resultant motion will be along the line A B, parallel to the length of the glacier. This result receives the most complete confirmation from observation, so that the drag towards the centre expresses only half the conditions of the problem.

To test the question on a small scale, the following experiment

Fig. 2.



was made, A B C D, (Fig. 2) is a wooden trough, six feet long, and one foot wide. The end, A C, of the trough is elevated. O is a

box, with a sluice front, containing fine mud, formed of a mixture of pipeclay and water. The mud was permitted to flow slowly through the trough. Two highly coloured straight lines *a b*, *c d*, were stamped on the mud transverse to the length of the trough, and two others *m n*, *o p*, were drawn parallel to its length. Now, if filaments move past each other towards the centre, such a motion must be observed by its effect upon the longitudinal lines *m n*, *o p*. On permitting the mud to flow, the lines lower down presented the appearance shown in the figure; *m n* and *o p* moved parallel to the sides, and at the same distance from them from top to bottom; there was no evidence whatever of the supposed drag: the differential motion which existed was parallel to the length of the trough, as is proved by the distortion of the lines *a b*, *c d*, while every point in each of the longitudinal lines moved exactly as the theoretical deduction from Fig 1 would lead us to expect. If then the cleaved structure were due to differential motion, we ought to expect it parallel to the sides, instead of oblique to them, as it actually exists in a glacier of the form typified by our wooden trough.

Finally, with regard to the transverse lamination of the centre of the glacier, the theory now under consideration assumes, that in a mass supposed to be viscous, with an enormous thrust behind, and an enormous resistance in front, fissures varying from a fraction of an inch to several inches in width are formed at right angles to the direction in which the thrust is exerted. Surely, so far from producing such fissures, the direct effect of such a force would be to close them up if they existed.

The speaker next attempted to apply the theory of slaty cleavage, already referred to, to the laminated structure of the ice. The lamination, like that of slate rock, is always approximately at right angles to the direction of maximum pressure; this fact is established by the testimony of independent observers, and was first, it was believed, recognised by Professor Forbes himself. Local circumstances, which give rise to a violent thrust, produce at the same time a highly developed lamination. When two confluent glaciers unite to form a single trunk, as in the case of the Finsteraar and Lauteraar glaciers, the effect of their mutual thrust is to develop the veined structure in a striking degree along their line of junction. The mechanical condition of such a glacier was illustrated by experiments with mud, flowing through two branches, which afterwards were united in a single trunk. Small red circles were stamped all over the surface of the mud in the two branches; and as these descended they were squeezed along the centre of the trunk into ellipses so elongated that the conjugate axis in many cases disappeared wholly, and the figure became a straight line. In nature it is exactly at the places where this squeezing takes place, that the cleavage of the ice is most highly developed; a fine example of this is the structure under the central

moraine of the Aar glacier. In fact, the association of pressure and lamination is far more distinct in the case of a glacier than in the case of slate rock. We know that in the latter case pressure is the sufficient cause of the lamination; are we not justified in concluding that it is also the cause in the former? The oblique position of the veins near the sides, the transverse lamination of the centre, the lenticular structure, the relation of the veins to the crevasses, are all in harmony with this compression theory of the veined structure of glacier ice. Unless, indeed, we suppose the compacted mountain snow to be perfectly homogeneous in a mechanical point of view,—a supposition plainly opposed to common sense—some portions of it will when under violent pressure, be rendered more compact than others, and the blue veins are the natural consequence.*

In the investigation, the well-known "dirt bands," to which so much theoretic importance is attached, were finally considered, and an explanation of these bands, as they are seen upon the glaciers of Grindelwald and of the Rhone, was attempted. On the former glacier the bands were particularly well developed, and a portion of the glacier where they did not exist was presented simultaneously with the bands upon another portion. Their proximate origin and final completion were thus observed at once; and to account for them the following explanation is proposed: these bands, wherever they have been observed, are, it is believed, first developed at the base of an ice cascade. The dirt, scattered by winds and avalanches over the upper regions of a glacier, is redistributed by the passage of the glacier through a precipitous gorge, where the ice is shattered and the dirt broken up into detached patches. On reaching the bottom, where the force becomes one of longitudinal compression,

* I have recently tried to reproduce the blue veins on a small scale by compressing snow. In some cases the section of the mass perpendicular to the surface, on which the pressure was exerted, exhibited in a feeble, but distinct manner, an appearance the same in kind as that of the veined structure of glacier ice. Stripes more transparent than the surrounding ice were observed at right angles to the direction of pressure. It is well known that the ice structure sometimes exhibits a true cleavage, and since the above discourse was given I have succeeded in impressing upon a perfectly transparent prism of ice a cleavage, the perfection of which surprised me. As in the case of the glacier and of slate rocks, the cleavage is perpendicular to the direction of pressure. On placing a specimen of the squeezed ice before a friend, he at first sight imagined it to be a bit of gypsum. The case then, as regards slaty cleavage and the structure of glacier ice, stands thus:—the testimony of independent observers proves that both ice and slate are laminated at right angles to the direction of pressure; and the question occurs, Is this pressure sufficient to produce the lamination? Experiment replies in the affirmative. I have reduced slate rock to an almost impalpable powder, and reproduced from it the lamination by pressure. The experiments above referred to prove the sufficiency of the pressure to produce the cleaved structure of the glacier ice. By combining the conditions of nature we have produced her results.

the patches of dirt are squeezed longitudinally and drawn out laterally, being thus converted into stripes of discoloration, which, owing to the speedier motion of the centre, are convex towards the lower extremity of the glacier. On consulting Professor Forbes's map of the Mer de Glace, it will be seen that the "bands" commence at the base of the icefall of the Talèfre, while none exist above the fall. Those shown on the Glacier du Géant, we are led to infer, commence at the base of the cascade of La Noire, which, however is not sketched on the map. The theory of Professor Forbes is, that a glacier throughout its entire length is composed of alternate segments of hard and porous ice, that the dirt is washed from the former, but finds a "lodgment" in the latter. The experiments on which this important conclusion is founded were unknown to the speaker, who finds observation and experiment in harmony with the explanation given above.

It may be urged that, after all, the foregoing experiments on the yielding of ice do not prove the viscous theory to be wrong. The mere fact of bending a prism of ice by fracture and regelation does not prove that it is non-viscous. This is perfectly true, nor was it conceived that the onus rested on the speaker to prove the negative here. All that was claimed for the experiments is the referring of certain observed phenomena to true causes, instead of to imaginary ones. An illustration may help to place this question in its true light. By Newton's calculation the velocity of sound was found to be one-sixth less than observation made it, and to account for this discrepancy he supposed that the sound passed instantaneously through the particles of air themselves, time being required only to accomplish the passage from particle to particle. He supposed the diameter of each air particle to be $\frac{173}{916}$ths of the distance between two particles; and nobody ever proved him wrong. Still, when Laplace assigned a *vera causa* for the discrepancy, the hypothesis of Newton, and other ingenious suppositions, were at once relinquished. The proof, indeed, in such cases consists in the substitution of *facts* for *conjectures*, and whether this has been done in the case now under consideration the intelligent reader must himself consider.

In the foregoing remarks, Mr. Tyndall has expressed his dissent from some of the theoretic views of Professor Forbes; but he feels great pleasure in recording the high value which he attaches to the experimental labours and observations of this philosopher. Indeed it is the fact of Professor Forbes having done so much that rendered such frequent reference to him necessary. To the same frank and friendly criticism the views propounded this evening were submitted; and a hope was entertained that the discussion of the question would result in a more exact acquaintance with the structure and motion of glaciers.

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 30.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Rev. F. D. Maurice, M.A. M.R.I.

Milton considered as a Schoolmaster.

Milton was an actual schoolmaster: his letter to Mr. Hartlib, explains his idea of education. In the year 1639, after his return from Italy, he took a house in St. Bride's Churchyard; afterwards one in Aldersgate Street, for the instruction, first, of his two nephews, and then of the children of some of his friends. According to Dr. Johnson, several of Milton's biographers have shown a desire to shrink from this passage of his life altogether, and have wished to represent his teaching as gratuitous. Johnson himself, while he ridicules this folly, sneers at Milton for returning to England, because his countrymen were engaged (as he thought) in a struggle for liberty, and then vapouring away his patriotism in a private boarding-school.

The earliest biographer of Milton, Edward Phillips, his nephew and pupil, is not open to the charge of regarding this occupation of Milton as a disgrace, or of hinting that he undertook it without remuneration. The others had probably had a notion that Aldersgate Street was not the place for a poet to dwell in, and that his work ought to be of a specially etherial kind. But Chaucer was Comptroller of Petty Customs, in the port of London; Spenser was born in East Smithfield, and died, it is to be feared, "for lack of bread," in King Street, Westminster; Shakespeare was busy at the Globe Theatre during the most important years of his life; and Milton himself was not only born at the Spread Eagle, in Bread Street, not only received his education at St. Paul's School, but had evidently a lingering love for London, whenever for a short time he was separated from it. There is clear evidence that he preferred the Thames to the Cam. Even in the genial years that he passed at his father's house in Horton, when he was writing "L'Allegro," "Il Penseroso," "Comus," "Lycidas," he was still paying frequent visits to London, that he might perfect himself in his father's favourite study of music, and in the mathematics. And

finally, he left Italy, where he had passed so many months of exquisite delight, and where he had received homage so unusual for any dweller on this side of the Alps, as soon as he heard of the probable meeting of the new (the Long) Parliament.

Johnson's complaint is refuted by his own sensible opinion that Milton taught for money, and not for amusement. Since he had determined that he ought to oppose the measures of the Court, was it not the duty of an honourable and prudent man to secure himself against the bribes of the Court? The patronage of Charles I. was bestowed with liberality and discernment. The report that a young man had come to London, who had received panegyrics from the academies and from the most eminent men of letters in Italy, was likely enough to reach the queen or the archbishop. There was nothing in Milton's previous career to render it improbable that he might be induced to use his pen in their favour. Instead of denouncing court entertainments, he had written a mask; he could be favourably spoken of by the family at Ludlow Castle. Money was important to him, for he had tastes that were expensive. No one would have felt more the charms of cultivated and refined society. Might not his scheme of the private boarding-school then be a very reasonable means of *preventing* him from vapouring away his patriotism, first by making him independent of his pen; secondly, by making him a less creditable associate for those who would have been glad to amuse themselves with his learning and eloquence?

We learn from Howell's "Londinopolis," printed in 1657, that Aldersgate Street "resembled an Italian street more than any other in London." Phillips speaks of it as "freer from noise than any other." Mr. Cunningham shows, in his Handbook, that it was the residence of distinguished noblemen. Milton must have strained a point to hire a house in such a situation. That he did so, is one sign of the earnestness with which he entered upon his task. We know, from his letter to Mr. Hartlib, that he regarded the building in which the education was conducted as a part of the education itself.

It is useless to speculate whether any of the friends to whom his letters or his sonnets are addressed committed their sons or kinsmen to his care. The names of John and Edward Phillips are all that have come down to us. Of these men, through the labours of Mr. Godwin, we have more information than it is generally possible to obtain respecting persons of their calibre. They were the younger brethren of that "fair infant whose death by a cough" is immortalised in one of Milton's early poems. When his sister married a second time, he took the boys into his house. Both became industrious literateurs. Both, even before the Restoration, became Royalists. Both for a time fell into the licentiousness which so commonly accompanied that reaction. John Phillips began with answering an anonymous reply to his uncle's defence;

then wrote a vulgar satire upon Presbyterians; became a travestier of Virgil; a dishonest translator of Don Quixote; a hack of the booksellers; in one discreditable passage, a reviler of Milton. No doubt the elevation of his uncle's character may have exasperated the grovelling tendencies in him. If he had been under the direction of a high-minded Royalist, he would probably have become a self-willed Puritan. The flogging of Busby would have been the most useful discipline for him. But he nowhere attributes his disgust at Puritanism to Milton's austerity. Edward Phillips, who shared that disgust, proves such a notion to be impossible. Nearly the last of his long series of books was the biography of his uncle. In it he recurs with affectionate reverence to the education he had received in Aldersgate Street, gives an account of that education, which shows that it embraced, as we might expect it would, every kind of study; that the tone of the teaching was noble, and that Milton knew when to unbend the bow as well as to nerve it. Edward Phillips speaks with warmth, and something of remorse, of the blessings which his school years might have been to him if he had passed them aright.

Johnson, who knew nothing concerning the Phillipses, except that one of them had written the "Theatrum Poetarum," speaks of the small fruit which proceeded from the "wonder-working academy" in Aldersgate Street. The fruits may have been unripe and unsatisfactory. Milton may have been disappointed in this as in his other hopes; other noble men have been so before and since. No one ever doubted that his own Samson was the image of himself; that the strong warrior became the blind and despised sufferer. But Samson was victorious in his death. There was a "Paradise Regained" as well as a "Paradise Lost" in Milton's history. His book on Education tells us what he learnt, and what we may learn by his school experiments. He never pretended that these worked any wonders; he does not even allude to them in his writings. His scheme of education certainly resembles in its principles that which Edward Phillips speaks of. It was not, therefore, a mere paper scheme; it referred to actual living boys, whom he had seen and tried to form. But the scale of it is one which he could never have attempted; and for aught that appears in the letter, he may have been led to it as much by a sense of his failures as by pride in his success.

In England we have grammar schools, and what are called commercial schools. In Germany there are gymnasia and real schools. The idea of the letter to Mr. Hartlib is, that this division is unnecessary and artificial, that the knowledge of words is best obtained in union with the knowledge of things; that each is helpful and necessary to the other. His maxim that "language is but the instrument conveying to us things useful to be known," might lead us to think that he did not regard language as a direct means of culture. This would be a hasty inference. He looked upon the

reading of good books as the best and only means of obtaining a knowledge of language. He protests, therefore, against "the preposterous exaction of forcing the empty wits of children to compose themes, verses, and orations," as a way to obtain a knowledge of the language. But the author of a host of Latin elegiacs, the Latin correspondent of foreign courts, is not so inconsistent with himself as to despise such exercises. He regards them as "the acts of ripest judgment, and the final work of a head filled by long reading and observing, with elegant maxims, and copious invention." This is not the language of a rebel against scholarship, but of a severe and fastidious scholar. His compassion for boys is combined with horror for their solecisms.

Milton's idea of education is strictly Baconian: not in this sense, that he had Bacon's preference for physical studies to humane or moral studies; but in this, that he protests against that method which starts from abstractions and conclusions of the intellect, and maintains that all true method must begin from the objects of sense. He may not have been well read in the "Novum Organum;" but he could not have applied its maxims more strictly in a new direction than he has done. Possibly his protests against making logic and metaphysics the introduction to knowledge in the Universities, when they ought to be the climax of knowledge, were more suitable to his own day, when boys went to Cambridge or Oxford at fifteen or twelve, than to ours. But if it be so, we ought to be very careful that our youths do acquire the early experimental training that he recommends, before they venture upon the higher and more abstract lore: otherwise we may have to complain, as he had, that "they grow into a hatred and contempt of learning," and that when "poverty or youthful years call them importunately their several ways, they hasten to an ambitious and mercenary, or ignorantly zealous divinity," or to the mere "trade of law," or to "state affairs, with souls so unprincipled in virtue and generous breeding, that court shifts and tyrannous aphorisms appear to them the highest points of wisdom," while "some of a more airy spirit live out their days in feasts and jollity."

Passing from his principles to his application of them, we may find abundant excuses for criticism, and, if we covet the reputation of wits, for ridicule. He wished his college to be both school and university; the studies therefore proceed in an ascending scale, from the elements of grammar to the highest science, as well as to the most practical pursuits. The younger boys are to be especially trained to a clear and distinct pronunciation, "as like as may be to the Italian." Books are to be given them like Cebes or Plutarch, which will "win them early to the love of virtue and true labour." In some hour of the day they are to be taught the rules of arithmetic and the elements of geometry. The evenings are to be taken up "with the easy grounds of religion, and the story of scripture." In the next stage they begin to study books on agriculture, Cato,

Varro, and Columella. These books will make them in time masters of any ordinary Latin prose, and will be at the same time "occasions of inciting and enabling them hereafter to improve the tillage of their country." The use of maps and globes is to be learnt from modern authors; but Greek is to be studied, as soon as the grammar is learnt, in the "historical physiology of Aristotle and Theophrastus." Latin and Greek authors together are to teach the principles of arithmetic, geometry, astronomy, and geography. Instruction in architecture, fortification, and engineering, follows. In natural philosophy we ascend through the history of meteors, minerals, plants, and living creatures to anatomy. Anatomy leads on to the study of medicine.

The objections to some of these plans are too obvious to need any notice. No one will suppose that natural philosophy is to be learnt from Seneca, or agriculture from Columella. Every one will admit readily that his own amazing powers of acquisition led Milton to overrate the powers of ordinary boys. But it would seem a poor reason for not availing ourselves of the hints that he gives us, that we have means of following them out which he had not: a poorer reason still for not profiting by the warnings which he gives us against filling our pupils' heads with a mere multitude of words, that he perhaps asked them to take in more both of words and things than they would be able comfortably to carry. If he is an idealist, he is certainly also a stern realist. He would have us always conversant with facts rather than with names. He aims at the useful as directly as the most professed utilitarian. The pupils are to have "the helpful experiences of hunters, fowlers, fishermen, shepherds, gardeners, and apothecaries," to assist them in their natural studies. These studies are to increase their interest in Hesiod, in Lucretius, and in the Georgics of Virgil. The incentive for studying medicine is, that they may perhaps "save armies by frugal and expenseless means, and not let the healthy and stout bodies of young men under them rot away for want of this (medical) disçipline."

Two other objections have been raised by Dr. Johnson against this scheme of education. The first will, probably, not have great weight with the members of the Royal Institution, for it turns upon the comparative worthlessness of the physical sciences. The other is expressed in some very elegant sentences, maintaining that the formation of a noble and useful character is the true end of education. One cannot help deploring that maxims so good and well-delivered should be so utterly thrown away. They are absurdly inapplicable to Milton's letter. It is throughout a complaint that the existing education was not sufficiently directed to the purpose of forming brave men and good citizens. It is throughout an assertion that that is the only purpose which any education ought to aim at. The classics are not resorted to for the purpose of forming a style, but of instilling manly thoughts, which a higher wisdom may purify

and make divine. Because the Englishman is a poor creature when he is busy with abstractions, and the strongest of all when he is dealing with realities, Milton would have him trained in these. All exercises and all recreations are to contribute to the same end. The pupils are to learn "the exact use of their weapon," both as "a good means of making them healthy, nimble, and well in breath, and of inspiring them with a gallant and fearless courage, which being tempered with seasonable precepts of true fortitude and patience, shall turn into a native and heroic valour, and make them hate the cowardice of doing wrong." In their very sports they are to learn the rudiments of soldiership.

Music is not recommended as a graceful recreation to a few, but as an instrument of making all the pupils "gentle from rustic passions and distempered passions."

Certainly, whatever the errors of Milton's system may have been, its ends were as noble and as practical as those of any that was ever conceived. An institution trained, as this is, to profit by the experiments of honest seekers in natural science, even if those experiments prove failures, will not despise the experiments of a moralist and a patriot who may have committed mistakes which the most ignorant may detect, who had a righteousness of purpose which the wisest will be most ready to admire and most eager to possess.

[F. D. M.]

GENERAL MONTHLY MEETING,

Monday, February 2.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

John Lister, Esq. and
Joseph Wood, Esq.

were duly *elected* Members of the Royal Institution.

Thanks were voted to Dr. Tyndall and Rev. F. D. Maurice, for their Discourses on January 23 and 30.

The special thanks of the Members were returned to Miss A. Savage for a handsomely bound copy of her father Mr. W.

Savage's work "On the Art of Decorative Painting," published in 1822, with additional Illustrations.

The following PRESENTS were announced, and the thanks of the Members returned for the same: —

FROM

Her Majesty's Government—Catalogue of Stars near the Ecliptic, observed at Markree, 1854-6. 8vo. 1856.

East India Company, the Hon.—Rig Veda Sanhita. Edited by Dr. Max Müller. Vol. III. 4to. 1856.

Lords of the Admiralty—The Nautical Almanac for 1857-60. 8vo.

Académie des Sciences de l'Institut Impérial de France—Mémoires présentés par Divers Savans. Tome XIV. 4to. 1856.

Mémoires. Tome XXVII. Partie 1. 4to. 1856.

Supplément aux Comptes Rendus. Tome I. 4to. 1856.

Actuaries, Institute of—Assurance Magazine. No. 26. 8vo. 1857.

Art-Union of London—Report for 1856. 8vo.

Almanacs for 1857. 8vo.

Asiatic Society of Bengal—Journal, No. 257. 8vo. 1855.

Astronomical Society, Royal—Monthly Notices. Vol. XVII. Nos. 1, 2. 8vo. 1856.

Babbage, Charles, Esq. F.R.S. (the Author)—Analysis of the Statistics of the Clearing-house during 1839. 8vo. 1856.

Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for Jan. 1857. 8vo.

Boosey, Messrs. (the Publishers)—The Musical World for Jan. 1857. 4to.

British Architects, Royal Institute of—Proceedings in Jan. 1857. 4to.

British and Foreign Bible Society—Catalogue of their Library, by G. Bullen. 8vo. 1857.

Carpenter, W. B. M.D. F.R.S. (the Author)—Researches on the Foraminifera. Part 2. 4to. (Phil. Trans.) 1856.

Copland, James, M.D. F.R.S. (the Author)—On the Drainage and Sewage of London. 12mo. 1857.

De la Rue, Warren, Esq. F.R.S. M.R.I.—Engraving of Jupiter, as seen with a Newtonian Equatoreal of 13 inches aperture, Oct. 25, 1856.

Editors—The Medical Circular for Dec. 1856, and Jan. 1857. 8vo.

The Practical Mechanic's Journal for Dec. 1856, and Jan. 1857. 4to.

The Journal of Gas-Lighting for Dec. 1856, and Jan. 1857. 4to.

The Mechanic's Magazine for Dec. 1856, and Jan. 1857. 8vo.

The Athenæum for Dec. 1856, and Jan. 1857. 4to.

The Engineer for Dec. 1856, and Jan. 1857. fol.

The Literarium for Dec. 1856, and Jan. 1857.

Faraday, Professor, D.C.L. F.R.S.—Monatsberichte der Königl. Preuss. Akademie, Sept. und Okt. 1856. 8vo. Berlin.

Franklin Institute of Pennsylvania—Journal, Vol. XXXI. No. 5; and Vol. XXXII. Nos 3, 4, 5, 6. 8vo. 1856.

Gamgee, Joseph S., Esq. (the Author)—Researches in Pathological Anatomy and Clinical Surgery. 8vo. 1856.

On the Advantages of the Use of the Starched Apparatus in the Treatment of Fractures, &c. 8vo. 1853.

Reflections on Petit's Operation. 8vo 1855.

Osservazioni sul Regime Dietetico. 8vo. 1853-4.

Geographical Society, Royal—Proceedings, No. 6. 8vo. 1857.

Gladstone, Dr. J. H. F.R.S. M.R.I. (the Author)—Papers on Chemical Affinity, &c. 4to. and 8vo.

Glösener, M. (the Author)—Recherches sur la Télégraphe Electrique. 8vo. Liége, 1855.

Graham, George, Esq. (Registrar-General)—Report of the Registrar-General for Dec. 1856, and Jan. 1857. 8vo.

Hamilton, Sir Charles J. J. Bart. C.B. M.R.I.—Pauli Jovii Opera. fol 1578.
Landois, H. (the Author)—Causes de la Coloration des Corps, &c. 8vo. 1857.
Lewin, Malcolm, Esq. M.R.I. (the Author).—On the Government of Oude. 8vo. 1857.
Locke, Mr. W. (the Hon. Sec.)—Reports of the Ragged School Union, 1855-6. 8vo.
Ragged School Union Magazine. 8vo. 1856.
Londesborough, The Lord, K.H. M.R.I.—Miscellanea Graphica, No. 11. 4to. 1857.
Macilwain, George, Esq. M.R.I. (the Author)—Memoir of John Abernethy. 3rd. Ed. 8vo. 1856.
Madrid, Real Academia de Ciencias—Memorias. Tom. III. & IV. 4to. 1856.
National Life-Boat Institution—The Life-Boat Journal for 1852-56. 8vo.
Newton, Messrs.—London Journal (New Series), Dec. 1856, and Jan. 1857. 8vo.
Novello, Mr. (the Publisher)—The Musical Times for Dec. 1856, and Jan. 1857. 4to.
Pepys, John, Esq. M.R.I.—"Shall" and "Will," or Two Chapters on Future Auxiliary Verbs. By Sir E. W. Head, Bart. 16to. 1856.
Poey, M. André (the Author)—Several Tracts on the Meteorology, Earthquakes, &c., of Cuba. 1855-56.
Photographic Society—Journal, Nos. 49, 50. 8vo. 1856.
Rennie, George, Esq. F.R.S.—Report from the Mersey Inquiry Committee. 8vo. 1856.
Roxburgh, W. M.D. M.R.I.—The Confession of Faith, &c., of the Church of Scotland, &c. 16to. 1845.
Savage, Miss Anne—Practical Hints on Decorative Printing, with Illustrations engraved on Wood, and printed in Colours at the Type Press. By Wm. Savage. fol. 1822.
Scoffern, John, M. B. (the Author)—Philosophy of Common Life. 16to. 1857.
Society of Arts.—Journal for Dec. 1856, and Jan. 1857. 8vo.
Statistical Society—Journal, Vol. XIX. Part 4. 8vo. 1856.
Taylor, Rev. W. F.R.S. M.R.I.—Das Leben Jesu. (Embossed) 4to. Wien. 1850.
Selection of Scotch Songs (Embossed). 4to. 1844.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Sept. und Okt. 1856. 4to.
Yates, James, Esq F.R.S. M.R.I. (the Author)—On the Versification of Homer. 8vo. 1857.

WEEKLY EVENING MEETING,

Friday, February 6.

Sir Benjamin Collins Brodie, Bart. D.C.L. F.R.S.
Vice-President, in the Chair.

John Hall Gladstone, Ph.D. F.R.S. M.R.I.

On Chromatic Phenomena exhibited by Transmitted Light.

The origin of colour was first illustrated by some elementary remarks and experiments. It was laid down as a fundamental principle, that the colour of an object depends on its reflecting or transmitting those rays of light which are capable of producing the sensation of the said colour. The objection that a rose is red not only when viewed by red light, but when seen in colourless daylight, was answered by showing that a beam of colourless light from the electric lamp really consisted of very many coloured rays, and was resolvable by a prism into a red, orange, yellow, green, blue, indigo, and violet light. This, when received on a white screen, showed a brilliantly coloured spectrum, and brightly tinted objects appeared of their ordinary hue only when illuminated by the ray of the same colour. It was explained that the electric light closely resembles that of the sun, but that the light of the great luminary is deficient in certain rays, so that a prismatic spectrum formed by daylight is traversed by very thin dark lines, which have been mapped and designated A, B, C, D, &c. Most artificial lights contain certain coloured rays in excess, hence objects illuminated by them exhibit that colour more prominently than by daylight. The soda flame, for instance, consists almost wholly of certain yellow rays which are wanting in the sun's light, coinciding in refrangibility (as Mr. Crookes has shown) with the dark line D; hence red or blue objects illuminated by it appear black, and nothing is reflected from those which do appear luminous excepting a ghastly yellow.

Leaving reflected, and turning to transmitted light, it was seen that pieces of coloured glass, interposed in the beam of light from the electric lamp, stopped certain rays, while they allowed others to pass through; thus a red glass cut off all the blue end of the spectrum, while a smalt-blue glass divided the red end into several

luminous bands alternating with dark spaces. The same was true of coloured liquids, a solution of sulphate of indigo absorbing the orange and yellow rays, and giving a spectrum consisting of a red ray separated by a broad black space from the green, blue, and violet. An oxy-hydrogen lime light, covered successively by red, yellow, and blue bell-glasses, produced the same effect on the coloured diagrams and other objects around, as if the source of light had been alternately red, yellow, and blue; and the opacity of these glasses to certain rays, and their transparency to others, was further illustrated by burning spirit lamps, the wicks of which had been previously sprinkled with salt, under yellow and blue bell-glasses. The yellow glass appeared perfectly transparent to the light which it covered, but the blue did not suffer the least yellow to pass; indeed, the soda-flame under it seemed of a pale violet tipped with green. It was explained that cobalt (to which the colour of the blue glass is owing) absorbs all those rays which are about the dark line D of the spectrum, although it suffers those a little less or a little more refrangible to pass freely; hence a considerable portion of the yellow light of the sun will penetrate such blue glass, but the yellow of the soda-flame is absolutely stopped. As a converse experiment, sulphuret of carbon lamps were ignited under the yellow and blue glasses; when the blue cover appeared almost transparent and colourless, while the yellow was opaque to the blue light, transmitting only some greenish rays.

If white light be transmitted through two or more media successively, each of which has a different absorbing effect upon it, very unexpected results may be frequently obtained. This is true of combinations of coloured glasses, or of coloured liquids. A red solution of meconate of iron, for instance, appears black when seen through the blue solution of an ammoniacal copper salt. If a vessel, filled with the blue alcoholic solution of a cobalt salt, be immersed in a pale yellow solution of chromic acid, it appears to contain a deep red liquid. Green nitrate of chromium also becomes red, when looked at through the same yellow solution. Similarly when two coloured compounds are mixed together, which are incapable of entering into chemical combination, an unexpected colour will frequently result; thus, on adding a little blue sulphindigotate of potash to a solution of yellow chromate of potash, the result was green, but on adding a larger amount of the blue salt it changed to red. There was here no chemical change; yet how naturally might a chemist have received the unlooked-for colour as evidence of a new compound!

This experiment introduced the subject of dichromatism. A thin stratum of even a highly coloured liquid is almost destitute of colour; thus the bubbles formed on shaking acetate of iron, or more familiarly, port-wine, or porter, appear white. That the colour of a solution changes in intensity, becoming paler when diluted, and deeper when concentrated, is known to all. This is

the general rule; yet a solution of yellow chromate of potash appeared scarcely any paler when diluted with perhaps twenty times its bulk of water. Sometimes also a complete change of colour takes place; thus acetate of chromium, which was red, became green when considerably diluted with pure water: a few drops of cochineal, stirred up in a tall champagne glass filled with water, imparted a red tint to the upper wide portion, and a lavender tint to the lower and narrow portion. A neutral solution of litmus is blue, alkalies render this (as is well known) still more blue, boracic or carbonic acid changes it to a wine red, and other acids to a bright red: yet slightly acid litmus was exhibited of a pale purple hue, and alkaline litmus of a deep red colour. All these phenomena were stated to be dependent, not on any chemical action exerted by the water, but on the quantity of the colouring substance traversed by the light in its passage to the eye; the same solution appearing of different colours according to the thickness seen through, and a deep stratum of a dilute liquid having the same tint as a shallow stratum of the same liquid when strong. The speaker added, that this phenomenon had been fully described and explained by Sir John Herschel, who termed it Dichromatism, but that fresh instances of it were being constantly observed; indeed, after investigating some cases of it last summer, he had, during a tour on the continent, noticed a fruit sauce which constantly appeared at the hotel dinners in Bavaria and other parts of Germany, and was beautifully dichromatic, red and blue, with every intervening shade of purple. By this character he had traced its composition, and found the colour was due to the deep red cherries which were very abundant at that season. He had noticed the phenomenon likewise in some specimens of the ordinary wine, in essence of lavender, in the syrup of green-gage tart, as well as in some pure chemical substances, such as red prussiate of potash, meconate of iron, purple comenamate of iron, citrate of iron, sulphindigotic acid, and permanganate of potash.

The prism reveals the origin of all these chromatic phenomena. It shows that the different rays of the spectrum are capable of penetrating different distances into a coloured medium. Thus, if port wine be placed in a wedge-shaped glass vessel, and this interposed in the refracted rays in such a position that each coloured ray can fall upon the different thicknesses of the liquid, it will be found that all the rays of the spectrum can penetrate a thin stratum, but that as the liquid increases in depth all are absorbed except the least refrangible red. Hence the thin film of a bubble of port is colourless. If yellow chromate of potash be examined in a similar manner, it is found to cut off the blue and violet rays at once, and to transmit the less refrangible half of the spectrum with equal freeness whether the stratum be thin or thick. Hence it is that dilution scarcely diminishes the colour of this dissolved salt. If a wedge of cobalt-blue glass (which is dichromatic) be inter-

posed across the spectrum, either of the sun, or of the electric light, a remarkable configuration is observed, which shows that the luminous bands of orange-red, of pale green, of orange, of yellow, of blue, and of violet, are absorbed at different distances in the order above given, while the extreme red penetrates any thickness with almost undiminished brightness. Acetate of chromium, placed in a hollow glass wedge, was seen to transmit the red, orange, green, blue, and violet rays through a thin stratum; yellow was absorbed at once, violet very quickly, while the maxima of luminosity were in the extreme red, and about the junction of green and blue, which in the solar spectrum is marked by the dark line F. These blue and green rays, however, which are transmitted in such quantity at first that the solution appears green to the unaided eye, are gradually absorbed, while the red ray continues to penetrate the dense solution, which of course assumes a red colour. A solution of litmus was seen to transmit the red, green, and blue rays freely, the maximum of absorption taking place between the fixed lines C and D of the solar spectrum: the addition of an alkali made little alteration beyond facilitating the transmission of the blue ray; while an acid diminished though it did not entirely retard it, causing the admission at the same time of the orange ray, and shifting the maximum of absorption to between *b* and F. As the red ray passes apparently unchanged through a great thickness of any of these solutions, neutral, alkaline, and acid litmus, all appear red if seen in sufficient quantity; indeed, paradoxical as it may sound, alkaline litmus is then of a purer red than acid litmus, since the latter transmits some orange light as well. The various appearances of the prismatic spectrum, as seen through these liquids in wedge-shaped vessels may be easily copied by a draughtsman; and, in fact, coloured diagrams were displayed, representing the prismatic images given by most of the above-mentioned substances. Some of these presented very characteristic forms: thus, cochineal showed two maxima of transmissibility, about B and G, penetrating far into the liquid, and two others in the green space, which, however, were speedily absorbed. Tincture of lavender, too, gave a spectrum marked by absorption bands, which seemed to coincide with the lines *b*, F, and G of the solar spectrum, though

Salt of Chromium.

Cochineal.

broader than these: the violet and green were more quickly absorbed than the blue rays, and these more quickly than the orange and red. The speaker observed, that all the solutions of chromium salts which he had examined, whether green, blue, or red, gave a prismatic image of the same form, —that described above,—the only perceptible difference being in the relative luminosity of the different colours: thus, on examining the green and blue modifications of nitrate of chromium in solutions of the same strength, the green in the first case appeared brighter than the blue, and penetrated to a somewhat greater distance, while in the second case it was the blue that had the advantage in luminosity; but the general configuration of the prismatic image was identical in the two modifications so different in appearance to the unaided eye. This was not the only instance in which the prism had revealed a wider application to the general rule, that a particular base or acid has the same, or very nearly the same effect, upon the rays of light, with whatever it may be combined. When two colouring substances combine, each continues to exert its proper influence on the various rays; thus, acid chromate of copper is yellowish green, because the chromic acid absorbs the blue and violet rays, and the copper the red ray, and thus orange, yellow, and green are alone transmitted. The diagrams also explained the production of first green, and afterwards red, on the admixture of sulphindigotate and

Lavender.

Sulphindigotate of Potash.

Chromate of Potash.

chromate of potash. Sulphindigotic acid and its salts admit the extreme red freely, but absorb the orange at once, the yellow very speedily, the green not so soon, and admit the blue and violet to a considerable distance: these last, however, are completely absorbed by chromic acid and the chromates; thus, a little red and much

green pass through a thin stratum of the mixed salts, while red alone is transmitted by a thick stratum.

Sir David Brewster observed that some coloured media caused a ray of a certain angle of refraction to appear of a different colour to that which it exhibited in the normal spectrum; and, mainly on these observations, he founded his remarkable theory that the prismatic spectrum consists of three superimposed spectra of the same length, one red, another yellow, and the third blue, which are coincident in position, but have their maxima of luminosity at different places. Some eminent philosophers of our own and other lands have denied not merely the conclusions, but even the observations of Brewster. Dr. Gladstone, however, could add his fullest testimony to the truth of the statement, that absorbent media frequently produce an apparent change of colour in a transmitted ray; and that not merely when a slit in the window-shutter is viewed by a prism through the interposed medium, but also when the altered prismatic spectrum is thrown upon a white screen. He had tried the latter experiment by means of light derived from the sun, from the electric lamp of the Royal Institution, and from the oxy-gas lime lamp of Mr. Highley, with the kind assistance of that gentleman, and always with a similar result. The large bell-glasses used during this discourse had been originally employed by Dr. Gladstone for experiments on the growth of plants, and he had then carefully examined the light transmitted through them. Through the blue glass he saw first a band of pure red light, then a dark space, then another luminous band which appeared to him like no colour of the spectrum, rather russet perhaps; his assistant called it "dirty chocolate;" a lady, who happened to come into the laboratory, unhesitatingly pronounced it "orange;" he was struck with this, as it certainly corresponded in position with the orange ray, though he did not know at that time, what has been frequently observed, that women are generally far more accurate in their appreciation of colours than men are. Accordingly he described the luminous band in his note-book as "orange, very bright, but unlike normal colour." Subsequently, he had found that Brewster called this second red ray in smalt blue glass "orange red;" but Herschel pronounced it "pure red;" while Helmholtz, isolating it from surrounding light, resolved it into its proper orange. Quite recently, on examining the prismatic spectrum thrown on a screen after traversing the same kind of glass, one scientific friend had called the second luminous ray "green," and another had designated it "brown," though on reconsideration each independently thought it had rather a reddish tint.

Thus Brewster's observation that a ray after transmission through certain absorbent media appears of a different colour to what it did before, is a truth. Yet the very fact that this colour seems so different to different eyes, and indeed to the same eye at different times, indicates that the phenomenon has a subjective rather than an ob-

jective origin. Difficulties of another character have also been urged against Brewster's deduction, by Helmholtz and others, and may be drawn from Maxwell's experiments. It is certain that changes in the apparent colour of a particular ray may arise from other causes than the absorption of one kind of light, while another kind having the same angle of refraction is transmitted. Of these may be enumerated :—First, an actual change of refrangibility, as in the cases of "fluorescence," so fully investigated by Professor Stokes. Secondly, a difference in the impression on the sense, arising from change of intensity. Thus blue, if very luminous, inclines to white, if faintly luminous to violet ; and so the fore-mentioned notebook designates the faint rays about F that were transmitted by the red bell-glass, "lilac," and the speaker had observed the blue in the prismatic spectra given by ammonio-sulphate of nickel, and by tincture of lavender, gradually shading off into violet as the light passed through deeper strata of liquid. The yellow of the solar spectrum appears to occupy a considerable space, if the sun be bright, but if diffuse daylight be examined, that space appears orange and green, while the yellow is perhaps confined to a very luminous line a little beyond D. It is not to be wondered at therefore that the green in the spectra of port wine and of citrate of iron, appears to invade the space usually occupied by the yellow, and the orange yellow. Yet in such cases the impression on different eyes may be very different ; thus, in rehearsing the experiments with the electric light at the Royal Institution, Dr. Gladstone had seen the bright space beyond D transmitted by blue glass of a decidedly green tint ; but Mr. Anderson had unhesitatingly called it yellow, its proper colour. This difference of sensation, arising from difference of intensity, was illustrated by the "Cercles chromatiques" of M. Chevreul, the first of which represents the bright colours of the spectrum, in which that called "*Jaune*" is certainly a beautiful yellow ; but the succeeding circles represent the same, reduced by the admixture of various percentages of black, and in them the "*Jaune*" becomes *green*, and so likewise does the "*Orange*," where a very large proportion of black has been added. A revolving disk, coloured black, on which had been fastened a segment of bright yellow paper, appeared uniformly green when set in rapid motion. Again, on one of Maxwell's colour tops was fixed an outer circle of red, and an inner one, partly black and partly orange ; when the top was spun the inner circle appeared green. Thirdly, contrast will frequently change the apparent colour of a particular ray. The result in the last experiment was partially due to this cause, the outer circle of bright red facilitating the sensation of its complementary colour green. Thus the dim light between D and E in the spectrum of ammonio-sulphate of nickel, with bright orange on one side and green on the other, assumes a very indefinite tint. The very remarkable prismatic image given by a solution of permanganate of potash in

the wedge-shaped vessel was exhibited, and it was seen that the orange band became very faint when the solution was deep, and in contrast with the neighbouring brilliant red appeared sometimes green, but more generally violet. Much, in this case also, was found to depend on the eye of the observer; but that a violet sensation might be produced from orange under such circumstances had been proved by the speaker, who in repeating one of Dr. Tyndall's experiments—that of looking at the daylight through a red glass on which a vermilion wafer was fastened—had frequently seen the wafer assume a violet tint. He believed that these three causes were sufficient to account for all the apparent changes of colour produced in a ray by absorbent media.

[J. H. G.]

WEEKLY EVENING MEETING,

Friday, February 13.

Sir Benjamin Collins Brodie, Bart., D.C.L. F.R.S.
in the Chair.

Thomas A. Malone, F.C.S.
DIRECTOR OF THE LABORATORY IN THE LONDON INSTITUTION.

On the Application of Light and Electricity to the production of Engravings—Photogalvanography.

The subject of this discourse is one with which the speaker has been for some years practically acquainted. In 1844, he experimented for many months upon the engraving process of M. Fizeau of Paris, in conjunction with M. Claudet and M. Fizeau. Since that time he has closely watched all the steps of improvement that have been taken, down to the latest investigations of Talbot, Niepce de St. Victor, Pretsch, and Poitevin. He ventured thus to think himself fairly entitled to lay before the auditory the numerous remarkable and beautiful specimens he had gathered, or kindly been furnished with, accompanied by such commentaries and notices of processes as the time admitted.

The various methods hitherto devised for the accomplishment of that important problem the certain perpetuation and cheap multiplication—by means of printer's ink and the ordinary printing presses—of the images of natural objects, as obtained in the camera obscura by the processes of ordinary photography, may be arranged under three great divisions.

The *first* method, in which light was used to aid the engraver's art was almost coeval with the first attempts made to produce sun-drawn pictures. Indeed it had been asserted that photography and photographic engraving were invented between the years 1813 and 1827, by one man, Nicéphore Niepce, of Chalon on the Saône. A reference, however, to the Journal of the Royal Institution* would show that photography really sprang from the labours of Thomas Wedgwood and Humphry Davy, as far back as the year 1802.

Although we cannot accord to Nicéphore Niepce the merit of originating photography, we must give him the undivided title of founder of the art of photographic engraving, and, moreover, acknowledge that he was the first to fix not only a *direct positive* photograph, but also to secure on metal and glass plates the images of the camera, and this long before Daguerre produced his wonderful plates. Of this there can remain no doubt, after a study of the remarkable specimens which Dr. Robert Brown has so kindly enabled photographers now for the first time publicly to examine. It was not generally known that Niepce's images of 1827 had so much that is beautiful, in common with the daguerreotype of a later date. Daguerre's pictures may be said to be only exalted examples of the same phenomenon: yet the processes are widely different. Niepce's method was beautifully simple, and as it gives us the ground-work of his etching process, must be briefly described. He took a bituminous substance called jew's pitch or asphaltum; upon this he poured oil of lavender to resolve the bitumen into a varnish with which he could coat plates of metal or glass. He used chiefly pewter and copper plated with silver. A plate coated and dried was exposed to the light with an engraving superimposed, or it was placed in the field of the camera obscura just as Wedgwood and Davy placed their prepared papers; and with a certain similarity of result, inasmuch as a photographic image was obtained on the varnished plate. This image, however, unlike that of Wedgwood and Davy, was *not visible*. The plate had to be submitted to the solvent action of a mixed liquid, composed of one part of oil of lavender and ten parts by measure of *white oil of petroleum*, or mineral naphtha. On immersion in this fluid the remarkable fact revealed itself, that wherever the light had acted the varnish had become insoluble, and in a certain degree proportionately so to the intensity of the light. There were not only lights and shadows but half tints. The picture, as soon as developed by the solvent, was removed, drained, and washed with water to check all further action. The shadows of the picture were now represented by the parts of the white metal, or glass plate laid bare; the lights were given by the film of varnish which the light had hardened, and the solvent had left untouched. The plate now finished was capable of being etched by simply pouring engraver's acid upon its surface. The

* Journal of the Royal Institution, Vol. I. p. 170.

varnish would protect the metal from the acid over the lights of the picture; while the shadows, represented by the bare metal, would be bitten in the manner common to all etching processes. On now removing the protecting varnish, the plate could be inked and printed from by the common copper-plate printing press. Such are the essential details of the first of the photographic engraving processes. The specimens on the table were presented in 1827 to Mr. Bauer, late of Kew, by Nicéphore Niepce, who for a short time resided at Kew, on a visit to a brother in infirm health. Niepce prepared a statement regarding his invention, for presentation to the Royal Society; but as he at that time kept his process secret, his manuscript was not published. Niepce appears to have returned to France, disappointed at his ill-fortune.

Niepce's bitumen process was improved by his nephew, M. Niepce de St. Victor, who has published a treatise* on it, giving the necessary minute instructions. The main features do not differ from those above given, though greater sensitiveness and perfection have been obtained. MM. Mante, Belloc, and Nègre, MM. Barreswil Davanne, Lerebours, and Lemercier have also advanced the bitumen process: the latter gentlemen having applied it to lithographic purposes. The process is still under trial; but the difficulties of obtaining a constantly uniform result at present stand in the way of its general adoption. It still deserves a thorough investigation.

The *second* method of producing photographic engravings is founded upon certain properties possessed by the Daguerrean image. It is found that a daguerreotype unfixed by gold is acted upon by nitric acid in its shadows, while the lights long resist the biting action of the acid. This is explained by assuming that the shadows are of pure silver, and that the lights consist of mercury —the acid attacking the silver by preference. The fact is, that an etching is obtained by merely leavng idiluted nitric acid in contact with the plate. The etched plate is then inked and printed from as in Niepce's case. Dr. Donné, of Paris, appears to have been the first to devise this method. Dr. Berres, of Vienna, also used nitric acid for this purpose; but the action is not easily controlled, and this form of the process has fallen into disuse. In 1842, Mr. Grove published in the Philosophical Magazine a method by which Daguerre's images can be engraved by the chlorine evolved by voltaic action, when the daguerreotype plate is made the positive terminal of the battery, and immersed in diluted hydrochloric acid; the negative wire being terminated by a plate of platinum, which was placed opposite and parallel to the photographic image. This process is much more under control than the last. [Prints from plates so engraved in 1842 were on the table.] This process is also

* Traité Pratique de Gravure Héliographique, par M. Niepce de Saint-Victor. Paris, Juin 1856.

worthy of further investigation. Here the image is truly drawn by light and *engraved by electricity*.

M. Fizeau, about the year 1844, also patented in this country, in conjunction with M. Claudet, a process for engraving the daguerreotype image. The speaker was instructed in this process by M. Fizeau, and worked for many months at its perfection. Results obtained both in France and England were upon the table, and showed that in cases where great delicacy of delineation was required, as in certain anatomical subjects, this process had not been surpassed. It quite justified the formation of a second division of the available photographic engraving processes.

M. Fizeau, like Mr. Grove, availed himself of the affinity of chlorine for silver, but relied on chemical action for its application. He (M. Fizeau) made a solution of common salt and nitrite of potash in water, to which he added nitric acid. This mixed acid acted immediately when aided by warmth, upon the silver of Daguerre's plate, and left untouched the parts supposed to be completely covered by mercury. Chloride of silver was thus at once formed in the shadows of the images, and after some time in the half tints also. A very faint etching was thus produced. A prolonged application of the acid would not further deepen the etching, since the insoluble chloride of silver at first formed protected the faintly etched parts from a further deepening corrosion. It was therefore necessary to remove the chloride of silver by washing with a solution of ammonia. This effected, the plate was ready for a second application of the acid, when chloride of silver would be again formed, to be once more removed by ammonia; and this alternation of solutions could be repeated a certain number of times, the etching increasing in depth at each operation. But in practice it was found that after a few applications of the acid the lights of the image also gave way, and thus the engraving came to an untimely end. To remedy this circumstance was M. Fizeau's great aim; and he succeeded in a marked degree by heating the etched plate in a strong and boiling solution of caustic potash, after which treatment the lights resisted well the injurious action they had before suffered from. It is not clear how the potash acts. M. Fizeau has supposed, and the speaker was inclined to support the view, that the potash acts merely as a hot bath, possessing a proper and a regular temperature which might restore the continuity of the amalgamated surface of mercury and silver as often as it was weakened to the point of breaking by the *under-biting* of the acid liquid. The heating in potash is an important feature in M. Fizeau's process. As soon as the etching has been carried as far as possible by the acid mixture, the plate is dried and inked with fine printer's ink, and an impression may be immediately taken; but M. Fizeau prefers that the ink should be allowed to dry in the hollows of the plate, the unetched parts being wiped clean, so that gold may be deposited only upon the bright parts by the electrotype process.

On now removing the ink, ordinary diluted nitric acid may be safely applied to the plate, to deepen still more the shadows without any danger of destroying the lights of the picture. This last step causes M. Fizeau's etchings to possess greater vigour than those obtained by Donné's or Grove's processes. The danger is, that under-biting may remove the half tints. However, some beautiful results obtained by the late Mr. Hurliman, a skilful engraver of Paris, attest the worth of this method. M. Fizeau, foreseeing that the wear and tear of the silver plates might be considerable, thought to use the electrotype process to produce fac-similes of the engraved plates, reserving the original plate unworn to supply any further demands, thus allowing any number of impressions to be struck off. Plates so electrotyped by the speaker twelve years ago, some of which were afterwards worked upon by an engraver, were placed upon the table. The patent right in this process will soon expire.

The processes of the *third* and last division were, it must be confessed, very desirable, notwithstanding the numerous satisfactory specimens obtained, and still to be obtained, by the processes previously described. The truth seemed to be that none of the processes gave uniformly satisfactory results: hence the necessity of being acquainted with the capabilities of all the chief known methods, and of impartially comparing them with a view to produce any special required result.

Mr. Henry Fox Talbot opens the third division by his method, known as the gelatine and bichromate of potash process, in which a steel plate is covered with a liquified jelly, containing bichromate of potash in solution. This jelly was allowed to dry upon the plate after the manner of Niepce's varnish; and the gelatined plate might be used in a similar way to reproduce engravings or the images of the camera; the light, as in Niepce's case, doing its work by altering and hardening the gelatine whenever it fell with sufficient intensity. On removing the plate from the light, and immersing it in water, it was found that the gelatine had become comparatively insoluble where the light had acted, but it retained its usual solubility over those parts which were in shadow. Thus the metal could be partially laid bare, as we have seen was the case in the bitumen process; the lights now would consist of the altered gelatine, and the shades be represented by the bare metal; it is evident we have only to pour an acid upon the plate to obtain an etching: but here some care and ingenuity will be required. Nitric acid acts so energetically and so uncertainly on the steel plate, that but little success would attend its employment. Accordingly, Mr. Talbot was led to seek a better engraving liquid. This was found in a solution of bichloride of platinum, which appeared to act in the desired manner. The advantage of any process on steel plates would be obvious, from the great number of impressions that so hard a body would yield under the wearing action of the printing press. It might here be observed that the bitumen process had

also been applied to steel plates by M. Mante, in a series of natural history plates, published in Paris, and also by M. Niepce de St. Victor, in the frontispiece to his treatise.

In 1854, Herr Paul Pretsch, of the Imperial printing office of Vienna, patented in this country, and subsequently in France, a process which he has called *Photogalvanography*. He uses Mr. Talbot's materials, but with certain additions, and avails himself of a property of the gelatine which allows of his dispensing with the acid etching altogether. We are unable to speak with certainty of the exact comparative merits and capabilities of the two processes. Mr. Talbot's results are on steel; Herr Pretsch's on copper. If other things be equal, the steel would possess the advantage of greater durability. Herr Pretsch takes one part of clear gelatine or glue, and about ten parts of water to form a jelly, which he mixes with a strong solution of bichromate of potash; to this mixture he adds a fresh portion of jelly, containing nitrate of silver in solution; the whole being warmed and thoroughly mixed for about ten minutes. He next adds a third portion of jelly, containing a comparatively small quantity of iodide of potassium; then the whole mixture is strained, and is ready to coat the glass plates which are at first used in this process. A plate being coated and dried, is applicable to all the purposes enumerated in the early bitumen process. It can be used to copy engravings by superposition, or be made to receive the images of the camera. However, it is found that the most practical way to make use of the bitumen and gelatine processes, is to copy from a positive photograph which has resulted from a collodion or a Talbotype negative. We have only to place the positive print upon the dried orange-coloured jelly, press it in contact by a plate of glass, and expose the whole to the light for some time, when we shall find upon removal that we have obtained upon the dried jelly a photographic representation of the positive print. Wherever the light has acted strongly the plate will have changed from its bright orange-red colour to a more tawny hue, this latter shade of colour gradually passing in the half tints into the unaltered red of the parts completely shielded from the light. The parts acted upon by the light have now become, as in Mr. Talbot's case, comparatively insoluble in water. So far, Herr Pretsch's process has much in common with Mr. Talbot's, but the two experimenters now *diverge* widely. Herr Pretsch, instead of dissolving away the unaltered jelly, merely soaks the plate in water long enough to cause the unaltered gelatine to swell, and so to rise above the surface in such a way that we obtain a picture in relief resembling the condition of an ordinary cut wood-block. The tawny coloured parts do not swell, and so they remain depressed, representing the sunken portions of the wood-block. If the swelled gelatine were hard enough, we might at once ink the raised parts by a roller, and print in the usual way. This, however, is impracticable; and, moreover, surface printing is

not in this art deemed to be the best mode of procedure. A device, analogous to one used in type printing, is therefore adopted; a sort of stereotype process is gone through. A mould in softened gutta percha, or other suitable moulding material,—possibly a composition of wax or stearine, is made; this mould will of course have the raised lines or dots of the original gelatine, represented by grooves and cavities, apparently graven in the surface; and here, again, if the mould were firm enough, we might ink it as if it were an engraved copper-plate, and print by the copper-plate printing press. From these considerations, it will be evident that we have only to seek to convert these yielding surfaces into enduring ones, and we shall end our labours successfully. This the electrotype art enables us to do. We have simply to render the mould a conductor of electricity, by black lead, or finely divided metal, and we can deposit in it copper to any amount. We shall thus get in copper a fac-simile of the original swelled gelatine plate. But since this requires surface printing, and that is not to be preferred, we must once more apply our electrotyping process, using this first obtained and raised copper-plate as a *matrix*, to produce as many engraved or sunken plates, ready to be printed from, as we may desire. The original matrix remains, as in Fizeau's case, unworn.

The above is an outline of the more important features of Herr Pretsch's invention. There is one more point that deserves attention. In all the engraving processes hitherto described, there is a difficulty in obtaining a granular surface over the etched parts necessary to hold the amount of ink required by the printer. In Pretsch's process this difficulty remarkably enough does not present itself; the swelled surface breaks up in a direction vertical to its surface into little masses which are just what is desired; this result is quite characteristic. It has been attributed to the presence of particles of chromate of silver, or of iodide of silver. Would it be too far-fetched to suppose that it is another beautiful instance of the slaty cleavage action demonstrated by Dr. Tyndall? However this may be, the fact is very important for the success of the invention. The chemistry of the processes of the first and third divisions of this subject is but little advanced. M. Niepce de St. Victor has found, what M. Chevreul had anticipated, that the oxygen of the atmosphere is essential in the bitumen process. In an illuminated vacuum the result could not be obtained, although ordinary photographic action went on quite as well as in air. With reference to the gelatine processes, it might be observed that Mr. Ponton, who first used bichromate of potash as a photographic agent, and M. Edmond Becquerel, who extended its use on paper, both found that the sizing materials became more insoluble by the photographic action. It was believed that chromic acid was liberated by the sun's rays, since simple mono-chromate of potash produced no effect. On mentioning these facts to a friend (Dr. Hugo Müller), the speaker learnt that solutions of chromium had been employed in

Germany, in experiments on tanning skins; and it therefore suggested itself that the chromic acid set free might, in re-acting on part of the gelatine, liberate an oxide of chromium, which, when combined with the rest of the gelatine, would form a species of artificial leather; thus rationally accounting for the comparative insolubility of the altered and tawny coloured portions of the jelly. The subject, however, requires and deserves a more thorough investigation.

M. Poitevin, of Paris, has applied the gelatine and bichromate of potash process to lithographic stone, and his results, placed on the table, would well bear a comparison with those obtained by the other methods described in this division.

The speaker, in conclusion, expressed his opinion that these engraving processes would greatly advance the art of photography itself, particularly in its applications to the delineation of coloured objects, in which it is still very imperfect, although some progress has been made.

[T. A. M.]

WEEKLY EVENING MEETING,

Friday, February 20.

Rev. John Barlow, M.A. F.R.S. Vice-President and Secretary, in the Chair.

Christopher Dresser, Esq.

On the Relation of Science and Ornamental Art.

The subject was introduced by a brief reference to the fact, that ornamental art is a necessity of man's nature; after which, one or two instances were brought forward in which science has directly aided ornamental art.

Chemistry was referred to as supplying many pigments; photography, as furnishing fac-similes of the choicest works of art of all ages; and "Nature Printing," as presenting the ornamentist with the flora of the entire world, which may be remodelled by him into æsthetic ornaments.*

After advancing these instances in which science has aided ornamental art, the object of the following remarks was set forth

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* A new process of "Nature-Printing" was here brought forward, and its supposed merits pointed out.

by one or two figures. The manner in which science has revealed the composition of light, and with this the laws of harmonious colouring was alluded to, with the power thus offered of imparting to the student the rules necessary to be attended to in order to produce agreeable effects by juxtaposing colours. It was then hinted that the object of the future remarks would be in a humble way to throw out a few suggestions relative to the laws which govern the aggregation of forms.

In continuation, it was observed that ornamental art consists of two elements, viz. design and manipulation. The former, which is chief, embracing construction and decoration.*

A few remarks were then offered upon construction, tending to show that a true principle of construction gives satisfaction, in the absence of which beauty cannot exist.

What is necessary, in order to produce beauty in construction, was then considered; but before proceeding to solve this question, the import of the term "beauty" was discussed. To the speaker's mind it conveyed some such idea as the following: that beauty is that quality in an object which causes a thrill of delight to pass through the soul of its beholder.

It was then observed, that in taking this view of beauty, all natural objects could not be considered as beautiful; as some strike the beholder as grotesque, and others as repugnant to all emotions of delight.

The term, beauty, having been thus considered, the subject was again recurred to, by calling attention to an idea that forms in architecture which certain members assume are the result of natural forces, as attraction, &c., and the more beautiful the object the more powerfully it appears to reveal the fact that it is the result of natural laws. The Greek vases were next alluded to, as presenting the appearance of being formed of a plastic material acted upon by the combined influences of the attraction of the earth and the centrifugal force; and the speaker considered that these combined influences appeared to a considerable extent to modify, or give rise to their forms. In these instances it was also endeavoured to show that where the laws of attraction have free room to act, or where nature herself modifies the form, beauty is gained. The character of the curves given to the boundary lines of bodies by the influences just alluded to was then noticed; and it was inferred, that from the very nature of the curves thus produced beauty must be gained.

Construction having been thus considered, decoration was noticed, and reference was made to a plant (the Dielytra spectabilis), in which it was pointed out that for the grace and flow of the larger members of the structure we are indebted to attraction, and that the operation of this influence upon arms, which would other-

* See Redgrave's Report on Design for the Exhibition of 1851.

wise be straight, made them graceful and beautiful. This principle was pointed out as apparently existing in favourite Greek ornaments.

Other principles were alluded to as existing in certain ornaments, as the "wave," "surface attraction," &c., which tended to show, that in order to gain beauty the decorating ornament must be intimately connected with the body decorated.

In conclusion, one or two other points were touched on, from which there was no endeavour made to extract broad principles.

[C. D.]

WEEKLY EVENING MEETING,

Friday, February 27.

H.R.H. Prince Albert, K.G. D.C.L. F.R.S. Vice-Patron, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On the Conservation of Force.

Various circumstances induce me at the present moment, to put forth a consideration regarding the conservation of force. I do not suppose that I can utter any truth respecting it, that has not already presented itself to the high and piercing intellects which move within the exalted regions of science; but the course of my own investigations and views makes me think, that the consideration may be of service to those persevering labourers (amongst whom I endeavour to class myself), who, occupied in the comparison of physical ideas with fundamental principles, and continually sustaining and aiding themselves by experiment and observation, delight to labour for the advance of natural knowledge, and strive to follow it into undiscovered regions.

There is no question which lies closer to the root of all physical knowledge, than that which inquires whether force can be destroyed or not. The progress of the strict science of modern times has tended more and more to produce the conviction that "force can neither be created nor destroyed;" and to render daily more manifest the value of the knowledge of that truth in experimental research. To admit, indeed, that force may be destructible or can altogether disappear, would be to admit that matter could be uncreated; for we know matter only by its forces: and though one of these is most commonly referred to, namely gravity, to prove its presence, it is not because gravity has any pretension, or any

exemption, amongst the forms of force as regards the principle of *conservation;* but simply that being, as far as we perceive, inconvertible in its nature and unchangeable in its manifestation, it offers an unchanging test of the matter which we recognize by it.

Agreeing with those who admit the conservation of force to be a principle in physics, as large and sure as that of the indestructibility of matter, or the invariability of gravity, I think that no particular idea of force has a right to unlimited or unqualified acceptance, that does not include *assent* to it; and also, to *definite amount* and *definite disposition of the force*, either in one effect or another, for these are necessary consequences: therefore, I urge, that the conservation of force ought to be admitted as a physical principle in all our hypotheses, whether partial or general, regarding the actions of matter. I have had doubts in my own mind whether the considerations I am about to advance are not rather metaphysical than physical. I am unable to define what is metaphysical in physical science; and am exceedingly adverse to the easy and unconsidered admission of one supposition upon another, suggested as they often are by very imperfect induction from a small number of facts, or by a very imperfect observation of the facts themselves: but, on the other hand, I think the philosopher may be bold in his application of principles which have been developed by close inquiry, have stood through much investigation, and continually increase in force. For instance, *time* is growing up daily into importance as an element in the exercise of force. The earth moves in its orbit in time; the crust of the earth moves in time; light moves in time; an electro-magnet requires time for its charge by an electric current: to inquire, therefore, whether power, acting either at sensible or insensible distances, always acts in *time*, is not to be metaphysical; if it acts in time and across space, it must act by physical lines of force; and our view of the nature of the force may be affected to the extremest degree by the conclusions, which experiment and observation on time may supply: being, perhaps, finally determinable only by them. To inquire after the possible time in which gravitating, magnetic, or electric force is exerted, is no more metaphysical than to mark the times of the hands of a clock in their progress; or that of the temple of Serapis in its ascents and descents; or the periods of the occultations of Jupiter's satellites; or that in which the light from them comes to the earth. Again, in some of the known cases of action in time, something happens whilst the *time* is passing which did not happen before, and does not continue after: it is, therefore, not metaphysical to expect an effect in *every* case, or to endeavour to discover its existence and determine its nature. So in regard to the principle of the conservation of force; I do not think that to admit it, and its consequences, whatever they may be, is to be metaphysical: on the contrary, if that word have any application to physics, then I think that any hypothesis, whether of

heat, or electricity, or gravitation, or any other form of force, which either wittingly or unwittingly dispenses with the principle of conservation, is more liable to the charge, than those which, by including it, become so far more strict and precise.

Supposing that the truth of the principle of the conservation of force is assented to, I come to *its uses.* No hypothesis should be admitted nor any assertion of a fact credited, that denies the principle. No view should be inconsistent or incompatible with it. Many of our hypotheses in the present state of science may not comprehend it, and may be unable to suggest its consequences; but none should oppose or contradict it.

If the principle be admitted, we perceive at once, that a theory or definition, though it may not contradict the principle cannot be accepted as sufficient or complete unless the former be contained in it; that however well or perfectly the definition may include and represent the state of things commonly considered under it, that state or result is only partial, and must not be accepted as exhausting the power or being the full equivalent, and therefore cannot be considered as representing its *whole nature;* that, indeed, it may express only a very small part of the whole, only a residual phenomenon, and hence give us but little indication of the full natural truth. Allowing the principle its force, we ought, in every hypothesis, either to account for its consequences by saying what the changes are when force of a given kind apparently disappears, as when ice thaws, or else should leave space for the idea of the conversion. If any hypothesis, more or less trustworthy on other accounts, is insufficient in expressing it or incompatible with it, the place of deficiency or opposition should be marked as the most important for examination; for there lies the hope of a discovery of new laws or a new condition of force. The deficiency should never be accepted as satisfactory, but be remembered and used as a stimulant to further inquiry; for conversions of force may here be hoped for. Suppositions may be accepted for the time, provided they are not in contradiction with the principle. Even an increased or diminished capacity is better than nothing at all; because such a supposition, if made, must be consistent with the nature of the original hypothesis, and may, therefore, by the application of experiment, be converted into a further test of probable truth. The case of a force simply removed or suspended, without a transferred exertion in some other direction, appears to me to be absolutely impossible.

If the principle be accepted as true, we have a right to pursue it to its consequences, no matter what they may be. It is, indeed, a duty to do so. A theory may be perfection, as far as it goes, but a consideration going beyond it, is not for that reason to be shut out. We might as well accept our limited horizon as the limits of the world. No magnitude, either of the phenomena or of the results to be dealt with, should stop our exertions to ascertain, by

the use of the principle, that something remains to be discovered, and to trace in what direction that discovery may lie.

I will endeavour to illustrate some of the points which have been urged, by reference, in the first instance, to a case of power, which has long had great attractions for me, because of its extreme simplicity, its promising nature, its universal presence, and its invariability under like circumstances; on which, though I have experimented* and as yet failed, I think experiment would be well bestowed: I mean the force of gravitation. I believe I represent the received idea of the gravitating force aright, in saying, that it is *a simple attractive force exerted between any two or all the particles or masses of matter, at every sensible distance, but with a strength varying inversely as the square of the distance.* The usual idea of the force implies *direct* action at a distance; and such a view appears to present little difficulty except to Newton, and a few, including myself, who in that respect, may be of like mind with him.†

This idea of gravity appears to me to ignore entirely the principle of the conservation of force; and by the terms of its definition, if taken in an absolute sense "*varying* inversely as the square of the distance" to be in direct opposition to it; and it becomes my duty, now, to point out where this contradiction occurs, and to use it in illustration of the principle of conservation. Assume two particles of matter A and B, in free space, and a force in each or in both by which they gravitate towards each other, the force being unalterable for an unchanging distance, but varying inversely as the square of the distance when the latter varies. Then, at the distance of 10 the force may be estimated as 1; whilst at the distance of 1, *i.e.* one-tenth of the former, the force will be 100: and if we suppose an elastic spring to be introduced between the two as a measure of the attractive force, the power compressing it will be a hundred times as much in the latter case as in the former. But from whence can this enormous increase of the power come? If we say that it is the character of this force, and content ourselves with that as a sufficient answer, then it appears to me, we admit a *creation* of power, and that to an enormous amount; yet by a change of condition, so small and simple, as to fail in leading the least instructed mind to think that it can be a sufficient cause:—we should admit a result which would equal the highest act our minds can appreciate of the working of infinite power upon matter; we should let loose the highest law in physical science which our faculties permit us to perceive, namely, the *conservation of force.* Suppose the two particles A and B removed back to the greater distance of 10, then the force of attraction would be only a hundredth part of that they previously possessed; this, according to the statement that the force varies inversely as the square of the distance would double the strangeness of the above results; it

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* Philosophical Transactions, 1851, p. 1. † See Note, p. 358.

would be an *annihilation* of force; an effect equal in its infinity and its consequences with *creation*, and only within the power of Him who has created.

We have a right to view gravitation under every form that either its definition or its effects can suggest to the mind; it is our privilege to do so with every force in nature; and it is only by so doing, that we have succeeded, to a large extent, in relating the various forms of power, so as to derive one from another, and thereby obtain confirmatory evidence of the great principle of the conservation of force. Then let us consider the two particles A and B as attracting each other by the force of gravitation, under another view. According to the definition, the force depends upon both particles, and if the particle A or B were by itself, it could not gravitate, *i.e.* it could have no attraction, no *force* of gravity. Supposing A to exist in that isolated state and without gravitating force, and then B placed in relation to it, gravitation comes on, as is supposed, on the part of both. Now, without trying to imagine *how* B, which had no gravitating force, can raise up gravitating force in A; and how A, equally without force beforehand can raise up force in B, still, to imagine it as a fact done, is to admit a creation of force in both particles; and so to bring ourselves within the impossible consequences which have already been referred to.

It may be said we cannot have an idea of one particle by itself, and so the reasoning fails. For my part I can comprehend a particle by itself just as easily as many particles; and though I cannot conceive the relation of a lone particle to gravitation, according to the limited view which is at present taken of that force, I can conceive its relation to something which causes gravitation, and with which, whether the particle is alone, or one of a universe of other particles, it is always related. But the reasoning upon a lone particle does not fail; for as the particles can be separated, we can easily conceive of the particle B being removed to an infinite distance from A, and then the power in A will be infinitely diminished. Such removal of B will be as if it were annihilated in regard to A, and the force in A will be annihilated at the same time: so that the case of a lone particle and that where different distances only are considered become one, being identical with each other in their consequences. And as removal of B to an infinite distance is as regards A annihilation of B, so removal to the smallest degree is, in principle, the same thing with displacement through infinite space: the smallest increase in distance involves annihilation of power; the annihilation of the second particle, so as to have A alone, involves no other consequence in relation to gravity; there is difference in degree, but no difference in the character of the result.

It seems hardly necessary to observe, that the same line of thought grows up in the mind if we consider the mutual gravitating action of one particle and many. The particle A will attract the

particle B at the distance of a mile with a certain degree of force; it will attract a particle C at the same distance of a mile with a power equal to that by which it attracts B; if myriads of like particles be placed at the given distance of a mile, A will attract each with equal force; and if other particles be accumulated round it, within and without the sphere of two miles diameter, it will attract them all with a force varying inversely with the square of the distance. How are we to conceive of this force growing up in A to a million fold or more? and if the surrounding particles be then removed, of its diminution in an equal degree? Or, how are we to look upon the power raised up in all these outer particles by the action of A on them, or by their action one on another, without admitting, according to the limited definition of gravitation, the facile generation and annihilation of force?

The assumption which we make for the time with regard to the nature of a power (as gravity, heat, &c.), and the form of words in which we express it, *i.e.* its definition, should be consistent with the fundamental principles of force generally. The conservation of force is a fundamental principle; hence the assumption with regard to a particular form of force, ought to imply what becomes of the force when its action is *increased* or *diminished*, or its *direction changed;* or else the assumption should admit that it is deficient on that point, being only half competent to represent the force; and, in any case, should not be opposed to the principle of conservation. The usual definition of gravity as *an attractive force between the particles of matter* VARYING *inversely as the square of the distance*, whilst it stands as a full definition of the power, is inconsistent with the principle of the conservation of force. If we accept the principle, such a definition must be an imperfect account of the whole of the force, and is probably only a description of one exercise of that power, whatever the nature of the force itself may be. If the definition be accepted as tacitly including the conservation of force, then it ought to admit, that consequences must occur during the suspended or diminished degree of its power as gravitation, equal in importance to the power suspended or hidden; being in fact equivalent to that diminution. It ought also to admit, that it is incompetent to suggest or deal with any of the consequences of that changed part or condition of the force, and cannot tell whether they depend on, or are related to, conditions *external* or *internal* to the gravitating particle; and, as it appears to me, can say neither yes nor no to any of the arguments or probabilities belonging to the subject.

If the definition *denies* the occurrence of such contingent results, it seems to me to be unphilosophical; if it simply *ignores* them, I think it is imperfect and insufficient; if it *admits* these things, or any part of them, then it prepares the natural philosopher to look for effects and conditions as yet unknown, and is open to any degree of development of the consequences and relations of power: by

denying, it opposes a dogmatic barrier to improvement; by ignoring, it becomes in many respects an inert thing, often much in the way; by admitting, it rises to the dignity of a stimulus to investigation, a pilot to human science.

The principle of the conservation of force would lead us to assume, that when A and B attract each other less because of increasing distance, then some other exertion of power, either within or without them, is proportionately growing up; and again, that when their distance is diminished, as from 10 to 1, the power of attraction, now increased a hundred-fold, has been produced out of some other form of power which has been equivalently reduced. This enlarged assumption of the nature of gravity is not more metaphysical than the half assumption; and is, I believe, more philosophical, and more in accordance with all physical considerations. The half assumption is, in my view of the matter, more dogmatic and irrational than the whole, because it leaves it to be understood, that power can be created and destroyed almost at pleasure.

When the equivalents of the various forms of force, as far as they are known, are considered, their differences appear very great; thus, a grain of water is known to have electric relations equivalent to a very powerful flash of lightning. It may therefore be supposed that a very large apparent amount of the force causing the phenomena of gravitation, may be the equivalent of a very small change in some unknown condition of the bodies, whose attraction is varying by change of distance. For my own part, many considerations urge my mind toward the idea of a cause of gravity, which is not resident in the particles of matter merely, but constantly in them, and all space. I have already put forth considerations regarding gravity which partake of this idea,* and it seems to have been unhesitatingly accepted by Newton.†

There is one wonderful condition of matter, perhaps its only true indication, namely *inertia;* but in relation to the ordinary definition of gravity, it only adds to the difficulty. For if we consider two particles of matter at a certain distance apart, attracting each other under the power of gravity and free to approach, they will approach; and when at only half the distance each will have had stored up in it, because of its *inertia*, a certain amount of mechanical force. This must be due to the force exerted, and, if the con-

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* Proceedings of the Royal Institution, 1855, Vol. II., p. 10, &c.

† "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance, through a *vacuum*, without the mediation of any thing else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent, acting constantly *according to* certain laws; but whether this agent be material or immaterial I have *left to the* consideration of my readers."—See Newton's Third Letter to Bentley.

servation principle be true, must have consumed an equivalent proportion of the cause of attraction; and yet, according to the definition of gravity, the attractive force is not diminished thereby, but increased four-fold, the force growing up within itself the more rapidly, the more it is occupied in producing other force. On the other hand, if mechanical force from without be used to separate the particles to twice their distance, this force is not stored up in momentum or by inertia, but disappears; and three-fourths of the attractive force at the first distance disappears with it: How can this be?

We know not the physical condition or action from which *inertia* results; but inertia is always a pure case of the conservation of force. It has a strict relation to gravity, as appears by the proportionate amount of force which gravity can communicate to the inert body; but it appears to have the same strict relation to other forces acting at a distance as those of magnetism or electricity, when they are so applied by the tangential balance as to act independent of the gravitating force. It has the like strict relation to force communicated by impact, pull, or in any other way. It enables a body to take up and conserve a given amount of force until that force is transferred to other bodies, or changed into an equivalent of some other form; that is all that we perceive in it: and we cannot find a more striking instance amongst natural, or possible, phenomena of the necessity of the conservation of force as a law of nature; or one more in contrast with the assumed variable condition of the gravitating force supposed to reside in the particles of matter.

Even gravity itself furnishes the strictest proof of the conservation of force in this, that its power is unchangeable for the same distance; and is by that in striking contrast with the variation which we assume in regard to the *cause of gravity*, to account for the *results* at different distances.

It will not be imagined for a moment that I am opposed to what may be called the *law of gravitating action*, that is, the law by which all the known effects of gravity are governed; what I am considering, is the definition of the *force* of gravitation. That the result of one exercise of a power may be inversely as the square of the distance, I believe and admit; and I know that it is so in the case of gravity, and has been verified to an extent that could hardly have been within the conception even of Newton himself when he gave utterance to the law: but that the *totality* of a force can be employed according to that law I do not believe, either in relation to gravitation, or electricity, or magnetism, or any other supposed form of power.

I might have drawn reasons for urging a continual recollection of, and reference to, the principle of the conservation of force from other forms of power than that of gravitation; but I think that when founded on gravitating phenomena, they appear in their

greatest simplicity; and precisely for this reason, that gravitation has not yet been connected by any degree of convertibility with the other forms of force. If I refer for a few minutes to these other forms, it is only to point in their variations, to the proofs of the value of the principle laid down, the consistency of the known phenomena with it, and the suggestions of research and discovery which arise from it.* *Heat*, for instance, is a mighty form of power, and its effects have been greatly developed; therefore, assumptions regarding its nature become useful and necessary, and philosophers try to define it. The most probable assumption is, that it is a motion of the particles of matter; but a view, at one time very popular, is, that it consists of a particular fluid of heat. Whether it be viewed in one way or the other, the principle of conservation is admitted, I believe, with all its force. When transferred from one portion to another portion of like matter the full amount of heat appears. When transferred to matter of another kind an apparent excess or deficiency often results; the word "capacity" is then introduced, which, whilst it acknowledges the principle of conservation, leaves space for research. When employed in changing the state of bodies, the appearance and disappearance of the heat is provided for consistently by the assumption of enlarged or diminished motion, or else space is left by the term "capacity" for the partial views; which remains to be developed. When converted into mechanical force, in the steam or air-engine, and so brought into direct contact with gravity, being then easily placed in relation to it, still the conservation of force is fully respected and wonderfully sustained. The constant amount of heat developed in the whole of a voltaic current described by M. P. A. Favre,† and the present state of the knowledge of thermo-electricity, are again fine partial or subordinate illustrations of the principle of conservation. Even when rendered radiant, and for the time giving no trace or signs of ordinary heat action, the assumptions regarding its nature have provided for the belief in the conservation of force, by admitting, either that it throws the ether into an equivalent state, in sustaining which for the time the power is engaged; or else, that the motion of the particles of heat is employed altogether in their own transit from place to place.

It is true that heat often becomes evident or insensible in a manner unknown to us; and we have a right to ask what is happening when the heat disappears in one part, as of the thermo-voltaic current, and appears in another; or when it enlarges or changes the state of bodies; or what would happen, if the heat, being presented, such changes were purposely opposed. We have a right to ask these questions, but not to ignore or deny the con-

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* Helmholtz, On the Conservation of Force. Taylor's Scientific Memoirs, 2nd Series, 1853, p. 114.

† Comptes Rendus, 1854, Vol. XXXIX., p. 1212.

servation of force; and one of the highest uses of the principle is to suggest such inquiries. Explications of similar points are continually produced, and will be most abundant from the hands of those who, not desiring to ease their labour by forgetting the principle, are ready to admit it either tacitly, or better still, effectively, being then continually guided by it. Such philosophers believe that heat must do its equivalent of work: that if in doing work it seem to disappear, it is still producing its equivalent effect, though often in a manner partially or totally unknown; and that if it give rise to another form of force (as we imperfectly express it), that force is equivalent in power to the heat which has disappeared.

What is called *chemical attraction*, affords equally instructive and suggestive considerations in relation to the principle of the conservation of force. The indestructibility of individual matter, is one case, and a most important one, of the conservation of chemical force. A molecule has been endowed with powers which give rise in it to various qualities, and these never change, either in their nature or amount. A particle of oxygen is ever a particle of oxygen—nothing can in the least wear it. If it enters into combination and disappears as oxygen,—if it pass through a thousand combinations, animal, vegetable, mineral,—if it lie hid for a thousand years and then be evolved, it is oxygen with its first qualities, neither more nor less. It has all its original force, and only that; the amount of force which it disengaged when hiding itself, has again to be employed in a reverse direction when it is set at liberty; and if, hereafter, we should decompose oxygen, and find it compounded of other particles, we should only increase the strength of the proof of the conservation of force, for we should have a right to say of these particles, long as they have been hidden, all that we could say of the oxygen itself.

Again, the body of facts included in the theory of definite proportions, witnesses to the truth of the conservation of force; and though we know little of the cause of the change of properties of the acting and produced bodies, or how the forces of the former are hid amongst those of the latter, we do not for an instant doubt the conservation, but are moved to look for the manner in which the forces are, for the time, disposed, or if they have taken up another form of force, to search what that form may be.

Even chemical action at a distance, which is in such antithetical contrast with the ordinary exertion of chemical affinity, since it can produce effects miles away from the particles on which they depend, and which are effectual only by forces acting at insensible distances, still proves the same thing, the conservation of force. Preparations can be made for a chemical action in the simple voltaic circuit, but until the circuit be complete that action does not occur; yet in completing we can so arrange the circuit, that a distant chemical action, the perfect equivalent of the dominant chemical action, shall be produced; and this result, whilst it establishes the electro-

chemical equivalent of power, establishes the principle of the conservation of force also, and at the same time suggests many collateral inquiries which have yet to be made and answered, before all that concerns the conservation in this case can be understood.

This and other instances of chemical action at a distance, carry our inquiring thoughts on from the facts to the physical mode of the exertion of force; for the qualities which seem located and fixed to certain particles of matter appear at a distance in connexion with particles altogether different. They also lead our thoughts to the *conversion* of one form of power into another: as for instance, in the *heat* which the elements of a voltaic pile may either show at the place where they act by their combustion or combination together; or in the distance, where the electric spark may be rendered manifest; or in the wire or fluids of the different parts of the circuit.

When we occupy ourselves with the dual forms of power, electricity and magnetism, we find great latitude of assumption; and necessarily so, for the powers become more and more complicated in their conditions. But still there is no apparent desire to let loose the force of the principle of conservation, even in those cases where the appearance and disappearance of force may seem most evident and striking. Electricity appears when there is consumption of no other force than that required for friction; we do not know how, but we search to know, not being willing to admit that the electric force can arise out of nothing. The two electricities are developed in equal proportions; and having appeared, we may dispose variously of the influence of one upon successive portions of the other, causing many changes in relation, yet never able to make the sum of the force of one kind in the least degree exceed or come short of the sum of the other. In that necessity of equality, we see another direct proof of the conservation of force, in the midst of a thousand changes that require to be developed in their principles before we can consider this part of science as even moderately known to us.

One assumption with regard to electricity is, that there is an electric fluid rendered evident by excitement in plus and minus proportions. Another assumption is, that there are two fluids of electricity, each particle of each repelling all particles like itself, and attracting all particles of the other kind always, and with a force proportionate to the inverse square of the distance, being so far analogous to the definition of gravity. This hypothesis is antagonistic to the law of the conservation of force, and open to all the objections that have been, or may be, made against the ordinary definition of gravity. Another assumption is, that each particle of the two electricities has a given amount of power, and can only attract contrary particles with the sum of that amount, acting upon *each* of two with only half the power it could in like circumstances

exert upon one. But various as are the assumptions, the conservation of force, (though wanting in the second,) is, I think, intended to be included in all. I might repeat the same observations nearly in regard to magnetism,—whether it be assumed as a fluid, or two fluids or electric currents,—whether the external action be supposed to be action at a distance, or dependent on an external condition and lines of force—still all are intended to admit the conservation of power as a principle to which the phenomena are subject.

The principles of physical knowledge are now so far developed as to enable us not merely to define or describe the *known*, but to state reasonable expectations regarding the *unknown;* and I think the principle of the conservation of force may greatly aid experimental philosophers in that duty to science, which consists in the enunciation of problems to be solved. It will lead us, in any case where the force remaining unchanged in form is altered in direction only, to look for the new disposition of the force; as in the cases of magnetism, static electricity, and perhaps gravity, and to ascertain that as a whole it remains unchanged in amount:—or, if the original force disappear, either altogether or in part, it will lead us to look for the new condition or form of force which should result, and to develope its equivalency to the force that has disappeared. Likewise, when force is developed, it will cause us to consider the previously existing equivalent to the force so appearing; and many such cases there are in chemical action. When force disappears, as in the electric or magnetic induction after more or less discharge, or that of gravity with an increasing distance; it will suggest a research as to whether the equivalent change is one within the apparently acting bodies, or one *external* (in part) to them. It will also raise up inquiry as to the nature of the internal or external state, both before the change and after. If supposed to be external, it will suggest the necessity of a physical process, by which the power is communicated from body to body; and in the case of external action, will lead to the inquiry whether, in any case, there can be truly action at a distance, or whether the ether, or some other medium, is not necessarily present.

We are not permitted as yet to see the nature of the source of physical power, but we are allowed to see much of the consistency existing amongst the various forms in which it is presented to us. Thus if, in static electricity, we consider an act of induction, we can perceive the consistency of all other like acts of induction with it. If we then take an electric current, and compare it with this inductive effect, we see their relation and consistency. In the same manner we have arrived at a knowledge of the consistency of magnetism with electricity, and also of chemical action and of heat with all the former; and if we see not the consistency between gravitation with any of these forms of force, I am strongly of the mind that it is because of our ignorance only. How imperfect would our idea of an electric current now be, if we were to leave

out of sight its origin, its static and dynamic induction, its magnetic influence, its chemical and heating effects? or our idea of any one of these results, if we left any of the others unregarded? That there should be a power of gravitation existing by itself, having *no relation to the other natural powers, and no respect to the law of the conservation of force*, is as little likely as that there should be a principle of levity as well as of gravity. Gravity may be only the residual part of the other forces of nature, as Mossotti has tried to show; but that it should fall out from the law of all other force, and should be outside the reach either of further experiment or philosophical conclusions, is not probable. So we must strive to learn more of this outstanding power, and endeavour to avoid any definition of it which is incompatible with the principles of force generally, for all the phenomena of nature lead us to believe that the great and governing law is one. I would much rather incline to believe that bodies affecting each other by gravitation act by lines of force of definite amount (somewhat in the manner of magnetic or electric induction, though without polarity), or by an ether pervading all parts of space, than admit that the conservation of force could be dispensed with.

It may be supposed, that one who has little or no mathematical knowledge should hardly assume a right to judge of the generality and force of a principle such as that which forms the subject of these remarks. My apology is this, I do not perceive that a mathematical mind, simply as such, has any advantage over an equally acute mind not mathematical, in perceiving the nature and power of a natural principle of action. It cannot of itself introduce the knowledge of any new principle. Dealing with any and every amount of static electricity, the mathematical mind can, and has balanced and adjusted them with wonderful advantage, and has foretold results which the experimentalist can do no more than verify. But it could not discover dynamic-electricity, nor electro-magnetism, nor magneto-electricity, or even suggest them; though when once discovered by the experimentalist, it can take them up with extreme facility. So in respect of the force of gravitation, it has calculated the results of the power in such a wonderful manner as to trace the known planets through their courses and perturbations, and in so doing has *discovered* a planet before unknown; but there may be results of the gravitating force of other kinds than attraction inversely as the square of the distance, of which it knows nothing, can discover nothing, and can neither assert nor deny their possibility or occurrence. Under these circumstances, a principle, which may be accepted as equally strict with mathematical knowledge, comprehensible without it, applicable by all in their philosophical logic whatever form that may take, and above all, suggestive, encouraging, and instructive to the mind of the experimentalist, should be the more earnestly employed and the more frequently resorted to when we are labouring either to discover new regions of

science, or to map out and develop those which are known into one harmonious whole; and if in such strivings, we, whilst applying the principle of conservation, see but imperfectly, still we should endeavour to see, for even an obscure and distorted vision is better than none. Let us, if we can, discover a new thing in *any shape;* the true appearance and character will be easily developed afterwards.

Some are much surprised that I should, as they think, venture to oppose the conclusions of Newton: but here there is a mistake. I do not oppose Newton on any point; it is rather those who sustain the idea of action at a distance, that contradict him. Doubtful as I ought to be of myself, I am certainly very glad to feel that my convictions are in accordance with his conclusions. At the same time, those who occupy themselves with such matters ought not to depend altogether upon authority, but should find reason within themselves, after careful thought and consideration, to use and abide by their own judgment. Newton himself, whilst referring to those who were judging his views, speaks of such as are competent to form an opinion in such matters, and makes a strong distinction between them and those who were incompetent for the case.

But after all, the principle of the conservation of force may by some be denied. Well, then, if it be unfounded even in its application to the smallest part of the science of force, the proof must be within our reach, for all physical science is so. In that case, discoveries as large or larger than any yet made, may be anticipated. I do not resist the search for them, for no one can do harm, but only good, who works with an earnest and truthful spirit in such a direction. But let us not admit the destruction or creation of force without clear and constant proof. Just as the chemist owes all the perfection of his science to his dependence on the certainty of gravitation applied by the balance, so may the physical philosopher expect to find the greatest security and the utmost aid in the principle of the conservation of force. All that we have that is good and safe, as the steam-engine, the electric-telegraph, &c., witness to that principle,—it would require a perpetual motion, a fire without heat, heat without a source, action without reaction, cause without effect, or effect without a cause, to displace it from its rank as a law of nature.

[M. F.]

GENERAL MONTHLY MEETING,

Monday, March 2.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Edward Richards Adams, Esq. M.A.
Lieut.-Col. F. St. Leger Alcocke.
Neil Arnott, M.D. F.R.S.
Major Lewis Burroughs.
John Clutton, Esq.
Edward Cotton, Esq.
Charles Alfred Elliott, Esq.
Rev. Robert Everest, F.G.S. F.Stat.S.
Frederick Gray, Esq.
Thomas Williams Helps, Esq. M.A.
Miss Elizabeth C. C. Latter.
George Matthey, Esq.
John Monk, Esq.
Dr. Alphonse Normandy.
Lady Pollock.
Rev. William Rogers.
Russell Scott, Esq.
Alexander Trotter, Esq.
William Trotter, Esq.
Mrs. Sarah Tomlinson.
Edward Vivian, Esq.
Richard Henry S. Vyvyan, Esq. and
Edward Orange W. Whitehouse, Esq.

were duly *elected* Members of the Royal Institution.

John Lister, Esq. and
Joseph Wood, Esq.

were *admitted* Members of the Royal Institution.

The following Presents were announced, and the thanks of the Members returned for the same :—

From—

Agricultural Society, Royal—Journal, Vol. XVII. Part 2. 8vo. 1857.
Astronomical Society, Royal—Monthly Notices, Vol. XVII. No. 3. 8vo. 1857.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for Feb. 1857. 8vo.

Boosey, Messrs. (the Publishers)—The Musical World for Feb. 1857. 4to.
British Architects, Royal Institute of—Proceedings in Feb. 1857. 4to.
British Meteorological Society—Report for 1856. 8vo.
De Brock, M. Ministre des Finances de Russie—Annales de l'Observatoire Physique Central de Russie, pour 1851–3. 4to. 1855–6.
Dublin Geological Society—Journal, Vol. VII. Part 3. 8vo. 1857.
Editors—The Medical Circular for Feb. 1857. 8vo.
The Practical Mechanic's Journal for Feb. 1857. 4to.
The Journal of Gas-Lighting for Feb. 1857. 4to.
The Mechanic's Magazine for Feb. 1857. 8vo.
The Athenæum for Feb. 1857. 4to.
The Engineer for Feb. 1857. fol.
Foxhall, Edward, Esq. M.R.I.—R. Fergusson's Illustrated Handbook of Architecture. 2 vols. 8vo. 1855.
T. Rickman's Gothic Architecture. 5th Edition. 8vo. 1848.
G. E. Street's Brick and Marble in the Middle Ages. 8vo. 1855.
Franklin Institute of Pennsylvania—Journal, Vol. XXXIII. No. 1. 8vo. 1856.
Graham, George, Esq. (Registrar-General)—Report of the Registrar-General for Feb. 1857. 8vo.
Johnson, E. M., Esq.—Algeria in 1856. By Viscount Cranborne. 8vo. 1857.
London University—Calendar for 1857. 12mo.
Newton, Messrs.—London Journal (New Series), Feb. 1857. 8vo.
Noad, H. M., Esq. F.R.S. M.R.I. (the Author)—Manual of Electricity. Part 2. 8vo. 1857.
Novello, Mr. (the Publisher)—The Musical Times, for Feb. 1857. 4to.
Perigal, Henry, Esq. (the Author)—The Moon Controversy—Facts and Definitions. 8vo. 1866.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1856. Heft 11, 12. 4to. Gotha, 1856.
Photographic Society—Journal, No. 51. 8vo. 1856.
Prince, C. Leeson, Esq. (the Author)—Meteorological Journal at Uckfield in 1856. 4to.
Royal Society of London—Proceedings, Nos. 23, 24. 8vo. 1857.
Smee, Alfred, Esq. (the Author)—The Monogenesis of Physical Forces. A Lecture at the London Institution. 8vo. 1857.
Society of Arts—Journal for Feb. 1857. 8vo.
Weale, J., Esq. (the Publisher)—Rudimentary and Educational Series. 15 Parts. 12mo. 1856.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Nov. und Dec. 1856. 4to.

sequence of the bell having a much greater power both of bearing blows and of giving out sound than usual; and if we knew nothing more about the matter than that there is one large bell in England which will advantageously bear a clapper twice as heavy in proportion as any other, it would be enough to show that there must be some essential difference between the constitution of that and other bells, which is worth investigating.

The art of bellfounding having sunk so low, as is indicated by what has taken place at the Royal Exchange, and by the great bell of York being not used at all, after having cost £2000, except having the hour struck upon it *by hand* once a-day, it was obviously necessary to begin at the beginning, as we may say, and take nothing for granted as proper to be adopted, merely because we find it in common use now. Accordingly, when I undertook the responsibility of determining the size, and shape, and composition of these five bells, the bellfounders having refused to take any responsibility beyond that of sound casting according to orders, the Chief Commissioner of Works authorised the making of such experiments as might be required before finally determining the design and composition of the bells. Those experiments have only cost about £100, a small sum compared with the value of this one bell, and quite insignificant compared with the importance of success or failure in a national work of this kind. I may observe also, that there is no reason to believe that the art of making large bells is at present in a more flourishing state abroad than here. All the foreign bells in the Great Exhibition of 1851 were bad. Sir Charles Barry and Professor Wheatstone were requested by the Board of Works to make inquiries on the subject at the Paris Exhibition in 1855; and it appears that there is no foreign bellfounder who has cast any bell above a quarter of the weight of the Westminster bell; and the proportions of copper and tin which were stated to be used by the one who has the highest reputation, M. Hildebrand, of Paris, differ from those which I am satisfied are the best, both from the analysis of old bells of great celebrity and from my own experiments. I am equally convinced, that the French shape of bells is not only not the best, but is not so good as what may be regarded as the standard English shape.

I have said already that you may get any depth of note out of a bell of any weight by making it thin enough. At first, everybody who hears a bell, like that which stood at the west end of the Exhibition of 1851, sounding with 29 cwt. very nearly the same note as our 16 ton bell, is ready to pronounce the common form of bell, with a *sound bow* of $\frac{1}{14}$th or $\frac{1}{15}$th of its diameter, a very absurd waste of metal. But did it ever occur to them to consider, how far they could hear that 29 cwt. hemispherical bell? It could not be heard as far as a common bell of 2 or 3 cwt.; and before you get to any great distance from a bell of that kind, the sound becomes thin and poor, and what we call in bell-founding language, potty.

Up to 7 or 8 inches, these bells do very well for house clocks, to be heard at a little distance; but nothing, in my opinion, can be worse than the bells of this shape, 2 or 3 feet in diameter, which people seem to be so fond of buying for the new fashioned cemeteries: whether from ignorance that they will sound very differently on the top of a chapel and in the bellfounder's shop, or because they think a melancholy and unpleasant sound appropriate, or because they want to buy their noise as cheap as possible, I do not pretend to say. These bells, and thin bells of any shape, bear the same kind of relation to thick ones, as the spiral striking wires of the American clocks bear to the common hemispherical clock bells; *i.e.* they have a deeper but a weaker sound, and, are only fit to be heard very near. A gong is another instrument in which a deep note, and a very loud noise at a small distance, may be got with a small weight of metal; but it is quite unfit for a clock to strike upon, not merely from the character of its sound, but because it can only be roused into full vibration by an accumulation of soft blows. Gongs are made of malleable bell-metal, about 4 of copper to 1 of tin, which is malleable when cooled suddenly.

The Chinese bells, some of which are very large, may be considered the next approximation towards the established form; for they are (speaking roughly) a prolate hemispheroid, but with the lip thickened; whereby the sound is made higher in pitch but stronger, and better adapted for sounding at a distance when struck with a heavy enough hammer. But still the shape of the Chinese bells is very bad for producing sound of a pleasing quality; and generally it may be said, at least I have thought so ever since I began bell-ringing twenty-four years ago, that all bells of which the slant side is not hollowed out considerably, are deficient in musical tone. The Chinese bells are not concave but convex in the slant side. None of the European bells are so bad as that; but all the French bells that I have seen, or seen pictures of, and the great bell of St. Peter's at Rome, of which a model is exhibited, are straighter in the side than ours. According to my observation, no bell is likely to be a good one unless you could put a stick as thick as $\frac{1}{12}$th of the diameter between the side or *waist* of the bell and a straight edge laid against the top and the bottom. There was a very marked difference between two of our experimental bells, which were alike in all other respects, except that one was straighter in the waist than the other, and that was decidedly the worst. This condition is generally satisfied by the English bells: indeed I think the fault of their shape is rather the contrary, and that they open out the mouth too much, as if the bell had been jumped down on a great anvil while it was soft, and so the mouth spread suddenly outwards. The shape which we adopted, after various experiments in both directions, is something between the shape of the great bell of Notre Dame, at Paris, (of which a figured section was sent over last year by the present architect

of the Cathedral,) and that of the great bell of Bow, which is probably much the same as that of St. Paul's, York, and Lincoln, as they all came from the same foundry in Whitechapel. Indeed, the sound-bow of this bell is fuller outside than the Paris bell, because it is thicker; so much so, that a straight edge laid externally against the top of the bell and the sound-bow would be thrown out beyond the lip; whereas generally such a straight line would touch the lip, and just clear the sound-bow. Only within the last few days I have found one other remarkable exception to this general rule of construction, and a remarkable coincidence with the external shape, and the proportions of height, breadth, and thickness of our bell, and that is no other than the great bell of Moscow, of which an exact section is given in Lyall's Russia, with various different versions of its weight. The inside shape, however, is not the same, and I am satisfied not so good, the curve being discontinuous, and presenting an angle just below where the clapper strikes, as in the Paris bell. That bell seems to have had a very short life, a large piece having been broken out in a fire the year after it was cast. Sir Roderick Murchison tells me that the sound of the Russian bells is remarkably sweet.

I cannot find that the exact height of a bell makes much difference. The foreign bells, except the Russian ones, it seems, are generally higher than ours, being nearly $\frac{5}{6}$th of their diameter high, whether you measure it vertically inside, or obliquely outside from the lip to the top corner, as the two measures are generally much alike on account of the curvature of the top or *crown*. Ours run from $\frac{2}{3}$rd to $\frac{3}{4}$th of the diameter, though there are some higher; and on the whole my impression is against the high ones. The vertical height inside of all these bells at Westminster is $\frac{11}{14}$ of the diameter. Lower than that, the bell does not look well; and I never saw an ugly bell that was a good one; and it is clear from all our experiments, that the upper or nearly cylindrical part is of considerable importance, and though its vibrations are hardly sensible, it cannot even be reduced in thickness without injury to the sound, of which we had a curious proof. A bell of the usual proportions, in which the thickness of the upper or thin part is one-third of the *sound bow* or thickest part, sounds a third or a fourth above the proper note when it is struck in the waist, and the sound there is generally harsh and unmusical besides. It occurred to both my colleague, the Rev. W. Taylor, and myself, that it would be better to make the waist thinner, so as to give the same note as the sound bow. After two or three trials we succeeded in doing this very nearly, and without reducing the waist below $\frac{1}{4}$th instead of $\frac{1}{3}$rd of the sound bow. The bell sounded very freely with a light blow, and kept the sound a long time, and a blow on the waist gave a much better sound than usual. But for all that, when we tried it at a distance with another bell of the same size and same thickness of sound bow, but a thicker waist, the thin one was manifestly the

worst, and had a peculiar unsteadiness of tone, and sounded more of what they call the harmonics along with the fundamental note, instead of less, as we expected.

But still we have to ascertain what should be the thickness of the sound-bow itself (which is often called for shortness the thickness of the bell). The large bells of a peal are sometimes made as thin as $\frac{1}{16}$th of the diameter, and by one of the modern bellfounders even thinner, and the small ones as thick as $\frac{1}{10}$th of the diameter. It is clear that the most effective proportion is from $\frac{D}{12}$ to $\frac{D}{13}$. In casting peals of bells it is necessary to take rather a wider range, in order to prevent the treble being so small and weak as to be overpowered by the tenor; though here I am convinced that the modern bellfounders run into the opposite error, and always make their large bells too thin. I know several peals in London in which the large bells are hardly heard when they are all rung, and are besides very inferior in quality to the others. Again, if you make the small bells too thick, for the purpose of getting a larger bell to sound the proper note, you approach the state in which the bell is a lump of metal too thick to have any musical vibration. This is a much less common fault than the other, because the nearly universal demand for as deep notes as can be got for the money is a strong temptation to make the thickest bells, *i.e.* the small ones, only just thick enough, and the large ones much too thin. Nothing can be more absurd than to spend from £300 to £800 on a peal of bells, which are merely got for the purpose of giving pleasure to those who hear them, and then insisting on their being made in a key which they cannot reach without being thin and bad and disagreeable. People evidently fancy they are getting more for their money by getting bells in a low key than a high one, whereas they are really getting less, inasmuch as they only get the same quantity of metal and have it spent in producing a bad article instead of a good one. The tenor of the new (third) peal at the Exchange is only 33 cwt., and sounds the same note, C, as that of Bow Church, which weighs 53 cwt. It is very evident that one of them must be wrong: you need only go and hear one strike eleven and the other twelve, and you will not have much doubt which it is. It is true that the tenor of the previous (second) peal at the Exchange, though still worse, was of the same weight, and as the founders alleged in their own defence, from the same patterns as Bow; but the bells must have been of bad metal, and some of them were certainly bad castings. The thickness of the Westminster bell was designed to be $\frac{2}{25}$th of the diameter, or 9 inches, which would have made it 14 tons, the weight which was prescribed for it twelve or thirteen years ago, long before I had anything to do with the bells or the clock. By some mistake in setting out the pattern, or making the mould, which the founders have never been able to account for, the bell was made $9\frac{3}{8}$ inches thick, which is very nearly $\frac{1}{12}$th of the diameter, 9 ft. $5\frac{1}{2}$ in., and which increased the weight to

16 tons, within 174 lbs., and raised the note from E flat to E. Fortunately the same ratio of increase was made throughout, and the waist is $3\frac{1}{3}$ in., or one-third of the sound-bow, as it ought to be; and therefore the only effect of the mistake is, that the bell is heavier and more powerful; for it being cast the first, the alteration of the note did not signify, as the four quarter bells can as easily be made to accord with E natural as with E flat. And as they will be rather smaller in consequence, the aggregate weight of the whole five will be about 24 tons, as I originally estimated. I have only to add, with reference to this part of the subject, that the width of the bell at the top inside is half the width at the mouth, as it generally is; though in some bells, for instance, the great clock bell at Exeter, it is the outside diameter that is made half the diameter at the mouth. It is of no use to state here the precise geometrical rules by which the pattern of a bell of what we now call the Westminster pattern is drawn, as they are purely empirical. I mean, that having got a bell, by trial, which we all agreed was better than any other, I made out some sufficiently simple rules for drawing the figure of its section by means of a few circles whose radii are all some definite numbers of 24th parts of the diameter of the bell: but there is no kind of *a priori* reason, that I know of, why a bell whose section or *sweep* is made of those particular curves, should be better than any other; and therefore I call the rules for tracing the curve merely empirical; and as they would be of no use to any one but bellfounders, who know them already, or easily may, if they like, I shall say no more on this part of the subject.

As I have been asked many questions about the mode of calculating the size of a bell, so as to produce a particular note, and the answer is very simple, I may as well give it, though it may be found already, with other information on this subject, in the only English book I know of which contains such information, I mean the second edition of my *Lectures on Church Building*, to which a chapter on bells is added. If you make eight bells, of any shape and material, provided they are all of the same, and their sections exactly similar figures (in the mathematical sense of the word), they will sound the eight notes of the diatonic scale, if all their dimensions are in these proportions—60, $53\frac{1}{3}$, 48, 45, 40, 36, 32, 30; which are merely convenient figures for representing, with only one fraction, the inverse proportions of the times of vibration belonging to the eight notes of the scale. And so, if you want to make a bell, a fifth above a given one—for instance, the B bell to our E, it must be $\frac{2}{3}$rd of the size in every dimension, unless you mean to vary the proportion of thickness to diameter; for the same rule then no longer holds, as a thinner bell will give the same note with a less diameter. The reason is, that, according to the general law of vibrating plates or springs, the time of vibration of similar bells varies as $\frac{\text{thickness}}{(\text{diameter})^2}$. When the bells are also completely similar

solids, the thickness itself varies as the diameter, and then the time of vibration may be said simply to vary inversely as the diameter. But for a recent letter in the *Times* from a Doctor of Music, who seems to have taken this bell under his special protection, it would have seemed superfluous to add that the size of the "column of air contained within a bell" has no more to do with its note, than the quantity of air in an American clock has to do with the note of the wire on which it strikes. You may have half a dozen bells of different notes, because of different thicknesses, all enclosing exactly the same body of air. I certainly agree with the opinion published by some of the bellfounders on a former occasion, that musicians are by no means necessarily the best judges of bells, except as to the single point of their being in tune with each other.

The weights of bells of similar figures of course vary as the cubes of their diameters, and may be nearly enough represented by these numbers—216, 152, 110, 91, 64, 46, 33, 27. But as we are now only concerned with the making of a single bell, I shall say no more on this point, beyond desiring you to remember that the exact tune of a set of bells, as they come out of the moulds, is quite a secondary consideration to their tone or quality of sound, because the notes can be altered a little either way by cutting, but the quality of the tone will remain the same for ever; except that it gets louder for the first two or three years that the bell is used, probably from the particles arranging themselves more completely in a crystalline order under the hammering, as is well known to take place even in wrought iron.

We may now consider the composition of bell-metal. It is so well known to consist generally of from 5 to 3 of copper to 1 of tin, that all the alloys of that kind are technically called bell-metal, whatever purpose they may be used for; just as the softer alloys of 8 or 10 to 1 are called gun-metal; and the harder and more brittle alloy of 2 to 1 is called speculum-metal. But you may wish to know whether it has been clearly ascertained that there is no other metal or alloy which would answer better, or equally well and cheaper. The only ones that have been suggested are aluminium, either pure or alloyed with copper; cast steel, the iron and tin alloy, called union-metal; and perhaps we may add, glass. The first is, of course, out of the question at present, as it is about 50 times as dear as copper, even reckoning by bulk, and much more by weight. I have not heard any large steel bells myself, but I have met with scarcely anybody who has, and does not condemn them as harsh and disagreeable, and having in fact nothing to recommend them except their cheapness; and as I said before, nothing can be more absurd than to spend money in buying cheap and bad luxuries. Much the same may be said of the iron and tin alloy, called union metal, of which there was a large bell in the Exhibition of 1851. It was said by Mr. Stirling, the patentee of that manufacture (though I understand the same alloy is described

by Rinmann, in 1784), that it did not answer to make bells of it with the sound-bow thicker than the waist, as usual; and if such bells are worse than the thin ones of that composition, I can only say they must be very bad indeed. I have seen also some cheap bells, evidently composed chiefly of iron, but I do not know what else, and they are much worse than the union metal bells. It is hardly necessary to say much of glass, because its brittleness is enough to disqualify it for use in bells; but besides that, the sound is very weak, compared with a bell-metal bell of the same size, or even the same weight, and of course much smaller.

There is another metal, which you will probably expect me to notice as a desirable ingredient in bells, that is silver. All that I have to say of it is, that it is a purely poetical and not a chemical ingredient of any known bell-metal; and that there is no foundation whatever for the vulgar notion that it was used in old bells, nor the least reason to believe that it would do any good. I happened to hear of an instance where it had been tried by a gentleman who had put his own silver into the pot at the bellfoundry, some years ago. I wrote to him to inquire about it, and he could not say that he remembered any particular effect. This seemed to me quite enough to settle that question. You may easily see for yourselves that a silver cup makes a rather worse bell than a cast-iron saucepan.

Dr. Percy, who has taken great interest in this subject, has cast several other small bells, by way of trying the effect of different alloys, besides the iron and tin just now mentioned. Here is one of iron 95, and antimony 5. The effect is not very different from that of iron and tin of the same proportions, and clearly not so good as copper and tin; and I should mention that antimony is generally considered to produce an analogous effect to tin in alloys, but always to the detriment of the metal in point of tenacity and strength. Again, here is a bell of a very singular composition, copper 88·65, and phosphorus 11·35. It makes a very hard compound, and capable of a fine polish, but more brittle than bell-metal, and inferior in sound even to the iron alloys. Copper 90·14, and aluminium 9·86, which makes the aluminium bear about the same proportion in bulk as the tin usually does, seemed much more promising. The alloy exceeds any bell-metal in strength and toughness, and polishes like gold; and as was mentioned in the lecture here on aluminium last year, it is superior to everything except gold and platinum in its resistance to the tarnishing effects of the air. This alloy would probably be an excellent material for watch wheels, the reeds of organ pipes, and a multitude of other things for which brass is now used—a far weaker and more easily corroded metal, but as yet much cheaper. But for all this, it will not stand for a moment against the old copper and tin alloy for bells; in fact, it is clearly the worst of all that we have yet tried. Here is also a brass

model for casting bells, which is of course a brass bell itself, and that is better than the phosphorus and aluminium alloys, though inferior to bell-metal. (These were all exhibited.)

So much for the compound metals that have been tried as a substitute for bell-metal. But we have now, through the kindness of M. Ste. Claire Deville, of Paris, who exhibited the mode of making aluminium here last year, the opportunity of realizing the anticipation then formed, from the sonorousness of a bar of aluminium hung by a string, and struck. He has taken great pains in casting a bell of this metal, from a drawing of our Westminster bell, reduced to six inches diameter. He has also turned the surface, which improves the sound of small bells, where the small unevennesses of casting bear a sensible proportion to the thickness of the metal, and in fact, has done everything to produce as perfect an aluminium bell as possible, though at its present price it can hardly be regarded as more than a curiosity. But now for the great question of its sound. I am afraid [ringing it] that it must be pronounced to exceed all the others in badness, as much as it does in cost. I cannot say I am much surprised; indeed you may see in the book I have referred to, that I did not expect it to be successful as a bell, any more than silver, merely because a bar of it will ring. But it was well worth while to try the experiment and settle it.

Still the question remains, what are the best proportions for the copper and tin alloy, which we are now quite sure, in some proportions, will give the strongest, clearest, and best sound possible? They have varied from something less than 3 to something more than 4 of copper to 1 of tin, even disregarding the bad bells of modern times, some of which contain no more than 10 per cent. of tin instead of from $\frac{1}{5}$th to $\frac{1}{4}$th, and no less than 10 per cent. of zinc, lead, and iron adulteration, as you may see in Ure's Dictionary, and other books. Without going through the details of the various experiments, it will be sufficient to say that we found by trial, what seemed probable enough before trial, that the best metal for this purpose is that which has the highest specific gravity of all the mixtures of copper and tin. It is clear, however, that the copper now smelted will not carry so much tin as the old copper did without making the alloy too brittle to be safely used. You will see from the table of analyses, which I shall give presently, that the Westminster bell contains less tin and antimony together, and more copper than the old bells of York Minster, and a great deal less tin in proportion to the copper than the famous bell of Rouen, which was broken up and melted into cannon in the first French revolution, and of which it is worth while to mention that it appears to have been commonly called the silver bell, though the analysis shows it had not a trace of silver in it. We found that the 3 to 1 alloy, even melted twice over, had a conchoidal fracture like glass, and was very much more brittle than 22 to 7 twice melted, or 7 to 2 once melted; and accordingly, the metal used for the Westminster

bells is 22 to 7 twice melted; or, reducing it for convenience of comparison to a percentage, the tin is 24·1 of the alloy (not of the copper), and the copper 75·86, which you see is very nearly the same as the result of the analysis of the bell when cast. This may seem extraordinary, because it is well known that the tin wastes more in melting than the copper; but no doubt the explanation of it is, that the antimony which comes out with the tin in the analysis goes in with the copper in the composition, unless special means are taken to eliminate it, which is not worth while, as antimony produces the same kind of effect as the tin, and a little of it does no harm; as we know from intentionally putting some into a small bell, though it is an inferior metal to tin both for bells and organ pipes, in which I understand it is frequently substituted to stiffen the lead, because the English organ builders will not use as much tin as the old ones did, and the German ones still do.

This 22 to 7 mixture, or even 3½ to 1, which is probably the best proportion to use for bells made at one melting, is a much "higher" metal, as they call it, than the modern bellfounders, either English or French, generally use. As there is no great difference in the price of the two metals, the reason why they prefer the lower quantity of tin is, that it makes the bells softer, and therefore easier to cut for tuning, which is obviously a very insufficient reason. I advise everybody who makes a contract for bells, to stipulate that they shall be rejected if they are found on analysis to contain less than 22, or at any rate 21 per cent. of tin, or more than 2 per cent. of anything but copper and tin.

ANALYSIS OF SEVERAL BELL-METALS.

	Rouen.	Gisors.	York.	Lincoln.	Westminster.	
			Old Peal.	1610.	Top.	Bottom.
Copper . . .	71·	72·4	72·76	74·7	75·31	75·07
Tin (with Antimony)	26·	24·2	25·39	23·11	24·37	24·7
Iron	1·2	..	·33	.09	·11	·12
Zinc	1·8	1·	..	traces.	..	..
Lead	..	·4	1·77	1·16	traces	traces
Nickel . . .	..	..	·85	·58	..	..
Specific gravity .			8·76 ..	8·78 ..	8·847 ..	8·869 8·94

The founders were afraid that by insisting on so much tin I should make the bell too brittle. I was satisfied that if they cast it properly it would not be so; and I shall now give some proofs of that. The first is, that the bell has now been rung frequently with a clapper from two to three times as heavy in proportion to the bell as all the other large bells in England, and pulled sometimes by as

many as ten men. Secondly, I have a piece of the bell, or rather of one of the runners at the top, which is always the least dense and the weakest part of the casting, about 2 inches square, and ·6 inch thick. I tried to break it in two with a 4 lbs. hammer on an anvil, both with and without the intervention of a cold chisel, and I tried in vain; whereas a piece of the Doncaster bell-metal, cast in 1835, which was exactly twice as thick, and therefore ought to have been four times as strong, broke quite easily under the first blow of the hammer, although it is at the same time softer, but of less specific gravity by something like 12 per cent., and visibly porous.

In fact, the metal of this bell is superior in this very important point of specific gravity to any bell-metal that I have examined, or have found any account of, and to the highest specific gravity which is given in any of the books for the densest alloy of copper and tin. The only exception to this remark is that, according to my weighing, the specific gravity of some small clock bells, made by a man of the name of Drury (who is now either dead or retired from business), was exactly the same as this, if not a little higher. But I do not profess to have done it with the same nicety as the bits of metal in this table (except the two first, which are taken from a book) were no doubt weighed with by Dr. Percy and Mr. Dick, at the Geological Museum, where also the analysis of this and the old Lincoln and York bells were made. And it is remarkable that there are no small clock bells to be got now, equal either in density or quality, to those of Drury's, who is believed to have had some secret mode of making them, as they contain nothing but the usual metals. It ought therefore to be made another condition with a bellfounder, that the specific gravity of his bells should not be less than 8·7; and this, you observe, is sensibly below any of the specific gravities in the above table, except the very bad metal of the Doncaster peal of 1835, which was always complained of as inferior to the old peal which it replaced, though the new peal was a heavier one. About a year ago, the founders of this bell were warned that it would not be passed by the referees, if the specific gravity came below this figure, at least unless we were so perfectly satisfied with its sound as to render further inquiry unnecessary; and I convinced them by a simple experiment, first, that it was easy enough to test the soundness of the casting without breaking it, and secondly, that such a thick casting would not be sound, or at any rate, not of proper density, unless the mould was made so hot as not to chill and set the outside of the metal too soon. I may add, that I knew before the weighing of the bits for specific gravity, that it must be high enough, from the gross weight of the bell, in proportion to its size and thickness; for if the specific gravity had been 8·7, instead of 8·9, the bell would have weighed 7 cwt. less,—a quantity quite large enough for calculation even in a bell of 16 tons. I remember that the man who came down from Mears's to examine the old Doncaster bells of 1722 for re-casting, underestimated the

weight of the tenor by 2½ cwt., no doubt judging of its weight according to what a bell of the same size and thickness would be when made of such metal as their new peal was.

This bell is also so elastic, that I can make the clapper of 13 cwt. strike both ways, pulling it alone, and therefore of course to one side only; which I never found the case with any other bell.

You will probably wish to hear something of the actual casting of the bell, which is by no means an easy operation, if we may judge from the much greater rarity of good large bells than of small ones. There was no bell in England above 3 tons weight, except perhaps the tenor of the peal at Exeter, equal to many that exist of half that weight. Sir Christopher Wren condemned and rejected the great bell of St. Paul's, for which the present was substituted in 1716; and that rejected bell was made by a founder whose bells, cast the same year as his St. Paul's bell, are still at St. Alban's, and are very good ones. The present St. Paul's bell is itself inferior to that of Bow and the old York Minster bells; and both the Lincoln and York Minster bells are feeble and unsatisfactory, though the same foundry, until the last 30 or 40 years, turned out many very good bells of smaller but yet considerable weight. The metal was twice melted, as it is for making speculums. It was first run into ingots of bell-metal in a common furnace, and then those ingots were melted and run into the mould from a reverberatory furnace, in which the fuel does not touch the metal, but the flame is carried over and reflected down upon it from the top, or dome over the melting hearth. The ingots were only in this furnace 2½ hours before the metal was ready for running, as the alloy of copper and tin melts, as usual with alloys, at a much lower heat than the most obstinate of the two metals requires alone; and the whole 16 tons were run into the mould in five minutes. I understand that quick casting is essential to the securing of sound casting.

Messrs. Warner make their moulds in a different way from usual. First of all a hollow *core* is built up of bricks, and straw, and clay, and made to fit the inside of the bell by being swept over with a wooden pattern or *sweep*, turning on a vertical axis through the middle of the core. For bells of moderate size, they keep a number of different sized cores of cast iron, instead of building them up of bricks; and the iron cores are covered with the loam as before. They are easily lifted into a furnace to be dried and heated, whereas the brick ones must have the fire lighted within them. But the great difference is in the outside mould, or *cope.* Generally a clay bell is made on the top of the core, the outside being turned by another sweep turning on the same vertical axis; and when this is dry, a third fabric of clay and straw is laid on the outside of the clay bell, and this is called the cope. When it is dry it is lifted off, and the clay bell broken away; the cope is then *put on again,* and the metal poured in where the clay bell was.

Not only is this a very roundabout process, but without great care in putting the cope on again, the bell is apt to come out not uniform in thickness all round. I have seen broken bells twice as thick on one side as the other. Messrs. Warner's plan is to make the cope of iron larger than would fit the bell; that is lined with the casting loam, which is turned by an inside instead of an outside sweep, and the junction being between an iron plate at the bottom of the core, and the flanch at the bottom of the cope, they can be fitted together more accurately than the clay core and cope can be, and moreover bolted together, so as to resist the bursting pressure of the melted metal, instead of having to rely merely on the sand with which the pit is filled, and such weights as may be laid upon it. The core and cope were both made very hot before the pit was closed in with sand; for that is still necessary to prevent too rapid cooling, which makes bell-metal soft, and what you may call rotten in texture, and indeed if it is rapid enough, will make it malleable. This bell was kept in the pit 12 days before the sand was taken out, and even then the cope was too hot to touch, and it was left two days more before it was taken off. It has now changed its colour so much from the effect of the London damp and air, that you must trust to my statement, that until it came here it presented that peculiar mottled appearance which is so much admired in organ pipes, rich in tin; in fact, a gentleman who came to look at it immediately remarked its "fine silvery hue," with that inveterate propensity to discover silver in bell-metal which seems to defy all chemical refutation. It is remarkable that the tin does not show itself in this way, if it is less than about $\frac{4}{13}$ of the copper, *i.e.*, about 23 per cent. of the alloy.

I have now told you all that is likely to be interesting about the construction of this bell, so far as its shape and composition affect the sound. But the description would be incomplete without a short notice of another feature in the design, very subordinate indeed to those which I have yet spoken of, but still not insignificant: I mean the construction of that part of the bell by which it is to be hung. The common, indeed I may say, the universal method, for no other has been ever used for large bells, is to cast six ears or loops on the top or crown of the bell, which are technically called *canons*, and through which certain iron hooks and straps are put to fasten the bell to the stock. Small bells may be securely enough hung by a single canon or plug with a hole in it, like the common house or hand bells, or in any equivalent way. This method of hanging by canons had long appeared to me unsatisfactory on account of its weakness; for not only has this metal no very great tenacity lengthwise, but the canons are always the weakest part of the casting, from being nearest to the top; and, I believe, there are few old peals in the kingdom in which some of the bells have not had their canons broken, and replaced by iron bolts put through holes drilled in the crown. Moreover, this

method of hanging makes it troublesome and expensive to turn the bell in the stock, to present a new surface to the clapper when it is worn thin in one place, and many bells have been cracked in consequence. A Mr. Baker took out a patent a few years ago for several new modes of hanging, for the purpose of enabling bells to be turned in the stock. The first is simply making a hole in the crown and hanging the bell by a single large bolt, which also spreads out into the staple to carry the clapper. The objection to this is, that nobody would like to trust the weight of a large swinging bell to a single bolt if he could use several instead; because, although a single bolt can of course be made large enough to carry anything, yet if there is any flaw or bad workmanship in it, the result would be something frightful with a large bell; at any rate, nobody who expressed an opinion about it on either of the two occasions when it was exhibited at the Institute of Architects, nor any one whom I have consulted about the making or hanging of the Westminster bells, nor indeed anybody anywhere whose opinion is worth mentioning, so far as I can learn, approves of such a mode of hanging a large bell like this, even though it does not swing, and therefore I declined Mr. Baker's invitation to adopt it. His other method, as described in a recent pamphlet and in his specification, is to cast a thickish pipe on the top of the bell, which is to go through the stock and be fastened with a large nut, just as his iron bolt was in the other plan: only the clapper bolt is now independent and goes through this pipe, and is held by another smaller nut on the top of it. This seems to me to combine the two vices of the weakness of canons and the risk of a single bolt in the most complete manner, with the addition of a thread cut on this bell-metal pipe, which is about as weak a construction as possible. I should think no person in his senses would use such a plan: in fact, Mr. Baker himself did not seem to contemplate using it, but only put it into his patent, as patentees do, with the object of securing possession of every possible new method of doing the thing in question they can think of: but as patentees also do sometimes, he left out at least one method which is better than those which he put in, and that is the following.

On the top of the bell is cast what has been called a button and a mushroom; and either name will do well enough, except that a mushroom has not a hole through it, and buttons have more than one. It is in fact a very thick short neck, with a strong flanch round the top, which is fastened to the stock, in moderate sized bells, merely by bolts with hooked ends; and in very large ones, by bolts passed through a collar, bolted together in two pieces. The clapper (if there is one) is hung by a separate bolt, which goes through the hole in the neck and through the stock; and it has nothing to do with carrying the weight of the bell, unless you like to make it with a shoulder, so as to help the outside bolts. By this method you hang the bell by a lump of its own metal as large as

you choose to make it; and besides that, when the bell is worn in one place, it can be turned round to present another after you have loosened the bolts a little. Clock hammers wear the surface of a bell so little compared with ringing, that these Westminster bells are not likely to want turning for 50 or 100 years, and therefore in this case that advantage is not of so much consequence as usual, or as obtaining the safest possible mode of hanging; but as the power of turning happens to be consistent with hanging the bell in the strongest way, we all agreed in adopting this, except that the founders rather regretted the loss of the canons as an ornamental finish to the bell. Anybody who has happened to read the aforesaid pamphlet, which Mr. Baker has very diligently circulated, will see his drawings of all the three methods, (I mean his own two patented methods, and my unpatented one,) and will see also that he has persuaded himself, after the manner of patentees, that my "mushroom" (the name which I think he himself gave it), held up under the stock by four or six bolts, is identical with his pipe going through the stock, and fastened on the top by a nut,—a point on which I have heard yet no opinion but one, that his own drawings are the best answer to his claim.

I shall conclude by giving you as complete a list as I have been able to make out, of all the large bells in the world, except in China, where the bells are of a different and inferior form. It is substantially the same as that given in the *Lectures on Church Building* before referred to, but with a few additions and corrections. I do not believe that the recorded weights of several large bells can be correct, because they are inconsistent with the dimensions, which are much more likely to be right. The bells of Sens and Exeter especially, cannot possibly weigh as much as is stated for them, viz. 15 tons and 5½ tons respectively. Indeed I am so convinced of that, that I shall put them in the table at 13 tons and 4½ tons, and I believe that will be above the real weight rather than below it. The Erfurt bell may, perhaps, be as heavy as is stated, because I believe it is a thick one; and from its celebrated quality, the specific gravity is certain to be high. I doubt whether the Paris bell is as heavy as that of Montreal, because its diameter is the same, and its thickness less throughout. To be sure, the specific gravity of the Montreal bell is probably no better than that of the late Doncaster bell-metal, from the same foundry; and therefore I have left the reputed weight in the table for the Paris bell, though from other calculations I still doubt its accuracy. On the other hand, I am certain that the weight of the two great Russian bells is very much underrated. There can be no mistake about the thickness of the large one, because a piece is broken out high enough for a man to walk through upright, and as I said before, the shape so nearly agrees with that of our bell, that the weight cannot be very different from that given by the ratio of the cubes of the diameters, and that would make it nearly 250 tons, which I

suppose is much the largest casting in the world. And the other Russian bell, being 18 feet wide, must be 110 tons, according to the Westminster scale, instead of 64, which is the recorded weight. I might have added several other Russian bells to the list, from Lyall's book, all of great weights, but it seemed hardly worth while, as everybody knows already that the Russians have surpassed all the world in the magnitude of their scale of bellfounding, and two or three instances prove as much as twenty. I have stopped the list at four tons. After these would come the single bells of Canterbury, Gloucester, and Beverley Minster, and the tenor bells of the peals of Exeter and York, St. Mary-le-Bow, St. Saviour's, and Sherbourne, which run from $3\frac{1}{2}$ to $2\frac{1}{2}$ tons.

List of Bells.

Bells.	Weight.		Diameter.		Thickness.	Note.	Clapper or Hammer.
	Tons.	Cwt.	Ft.	In.	Inches.		
Moscow, 1736 . . broken, 1737 . .	250	?	22	8	23	..	..
Another, 1817 . . .	110	?	18	0	..	..	$\frac{1}{35}$ of bell.
Three others . . .	16 to 31		..		..	..	..
Novogorod	31	0	..		..	..	..
Olmutz	17	18	..		..	..	..
Vienna, 1711 . . .	17	14	9	10	..	..	..
Westminster, 1856 .	15	$18\frac{1}{2}$	9	$5\frac{1}{2}$	$9\frac{7}{8}$	E.	12 cwt.
Erfurt, 1497 . . .	13	15	8	$7\frac{1}{2}$	..	F.	..
Paris, 1680	12	16	8	7	$7\frac{1}{2}$	..	$6\frac{1}{2}$ „
Sens	13	?	8	7	..	..	..
Montreal, 1847 . . .	12	15	8	7	$8\frac{1}{2}$	F.	..
Cologne, 1448 . . .	11	3	7	11	..	G.	..
Breslaw, 1507 . . .	11	0	..		..	..	..
Gorlitz	10	17	..		..	..	..
York, 1845	10	15	8	4	8	F sharp.	4 „
Bruges, 1680 . . .	10	5	..		..	G.	..
St. Peter's, Rome . .	8	0	..		..	..	..
Oxford, 1680 . . .	7	12	7	0	$6\frac{1}{8}$	..	80 lbs.
Lucerne, 1636 . . .	7	11	..		..	G.	..
Halsberstadt, 1457 .	7	10	..		..	..	..
Antwerp	7	8	..		..	..	..
Brussels	7	$1\frac{1}{2}$	..		..	G sharp.	..
Dantzic, 1453 . . .	6	1	..		..	..	..
Lincoln, 1834 . . .	5	8	6	$10\frac{1}{2}$	6	A.	150 „
St. Paul's, 1716 . .	5	4	6	9	..	A.	180 „
Ghent	4	18	..		..	..	..
Boulogne, new . . .	4	18	..		..	..	..
Exeter, 1675 . . .	4	10 ?	6	4	5	A.	75 „
Old Lincoln, 1610 .	4	8	6	$3\frac{1}{2}$	..	B flat.	..
Fourth quarter-bell, Westminster, 1857	4	0	6	0	$5\frac{3}{4}$	B.	..

[E. B. D.]

[redacted]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, March 13, 1857.

Sir Roderick I. Murchison, G.C.S. F.R.S. Vice-President, in the Chair.

Professor John Phillips, M.A. F.R.S. F.G.S.
READER IN GEOLOGY IN THE UNIVERSITY OF OXFORD,

On the Malvern Hills.

Eighteen hundred years have passed away since one of the greatest of the Roman soldiers stood on the brow of the Cotteswold hills, and gazed with longing eyes across the vale of Severn to the distant mountains which formed the last defence of Britain. Full in front rose the rocky chain of Malvern, crested with war camps, and defended by Caractacus. Little thought the contending warriors that in another age chiefs of a milder mood, wielding very different weapons, should bring into subjection that unexplored region, and make the names of Murchison and Sedgwick as famous as ever were those of Ostorius and Caractacus. And little thought the philosophic historian, who records the captivity of the Silurian chief, that the poor province of Britain which struggled so hard for liberty, should in another day become a kingdom of science, with a sway extending far beyond the bounds of the Roman Empire, and archives stretching back beyond the building of Rome and the origin of nations, to the early days of creation and the beginning of life upon the globe.

As plainly as the Annals of Tacitus preserve for us the steps of the Silurian war, so clearly the Silurian strata mark successive stages in the construction of the earth. The earth has records, history, and chronology. When, amidst the broken walls of some ruined abbey, we judge of the age of its various parts by the form of the arch and the mouldings of the windows, or examine trees rooted in Saxon soil before the advent of the Norman conqueror, and determine their date by counting the rings of annual growth, the process we employ is like that which is in every day use by the geologist. Antiquaries of a new order, as Cuvier justly describes himself, we learn to restore by a mental effort the long past events of nature, to collect and place in their true order the fragments of

the early history of the earth, and to compare them with the recognized phenomena of the existing world of matter and life.

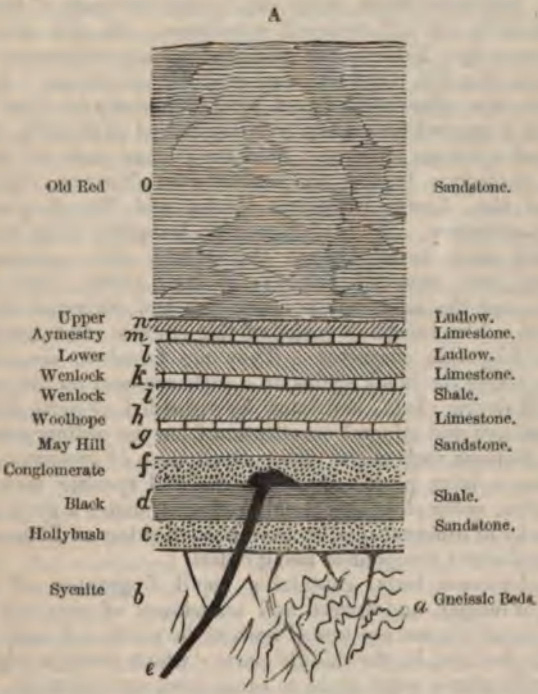
The Malvern hills stand nearly on a line of ancient boundary between different races of people. From the estuary of the Mersey to the mouth of the Severn, the elevated region on the west has always sheltered the truly British tribes; while on the east invaders of many names have alternately conquered and been dispossessed of the richer vales and gentler hills. Even more remarkable is their position in a geological point of view; they stand between monuments of two ages of the world: on the west, Palæozoic—on the east, Mesozoic deposits; on the west, an elevated region of undulated stratification—on the east, these strata thrown down by a vast fault, and covered by others of later date, lie against the Malvern hills as against a wall. So they stood between land and sea at the close of the Palæozoic period; between dry land full of trilobites and other organic monuments of the earliest ages of the world, and seas swarming with enaliosaurians and ammonites, and other forms of life never seen before that epoch, and long since removed from the catalogue of life.

The rocks which stand in this remarkable relation to physical geography are of igneous origin—that is to say, they have acquired their present characters as rock by consolidation from a state of fusion. The epoch when this occurred—the geological date of the rock—was probably earlier than all but the very oldest of the Palæozoic strata of this region; earlier than any of the Silurian strata on the west of the chain. The Malvern rock is then *one of the very oldest masses* of a granitic or syenitic nature which can be mentioned in the British Isles, or even in Europe; for in the greater number of other cases where granite is found below Palæozoic strata, it shows by veins injected, and by great metamorphisms adjacent, that it was in fusion after the consolidation of these strata. The date of the *elevation* of the Malvern rock is, however, not of the same antiquity, for it was, with the strata deposited on it, moved both upward and downward in the Silurian ages, and only acquired its full relative height by great flexures after the Devonian periods, and a great fault after the Permian ages.

If we now replace in imagination the rocks in the position they occupied before the occurrence of that great fault, and the still earlier disturbance indicated by the great anticlinal and synclinal flexures, we shall have the following vertical section of the strata of Palæozoic date. (A)

And, turning our attention to the very earliest effects of which traces remain, we shall reach a period earlier than the date of the fluidity of the syenitic rocks of Malvern. The evidence of this is found in many laminated rocks, with micaceous often twisted surfaces (*a*), which lie in the midst of the syenite about Malvern Wells, and Little Malvern, on the eastern face and near the foot of the hills. These limited tracts of mica schist, and gneiss, are to

be regarded as metamorphic rocks, whose actual appearance is due to the heat-influence of the melted syenites in which they are involved, but of whose earlier stratified state or date there is no certain evidence.



Next in order of date is the flow of syenitic rocks (*b*), using this term for a great variety of mineral compounds, in which felspar, quartz, hornblende, mica, and epidote are the most abundant. Some of these compounds may be termed granite, and such especially occur in veins which ramify among the hornblendic rocks; others are a beautiful mixture of felspar and hornblende; others, nearly pure felspar; others, masses of mica or hornblende. The composition of these rocks varies from hill to hill.

The upper surface of this flow was uneven. On the southern part of the new sea bed thus constituted was deposited a thick mass of rather greenish sandstone (*c*), containing impressions of marine plants, but no other organic remains. This deposit seems to have happened not long after the flow of the syenite, for it is indurated and somewhat altered along and near to the surfaces of contact. This change is best seen at the end of the Raggedstone

Hill, where a narrow boss of the rocks of fusion is covered by the sandstone.

After the formation of this sandstone, the sea bed in the Malvern district probably experienced a great depression; for the next deposit, laminated black shale (*d*), some hundreds of feet thick, indicates deep sea and tranquil subsidence. In this shale, some twelve years ago, I found the minute trilobitic crustaceans known as Olenus humilis, O. bisulcatus and O. spinulosus. Agnostus pisiformis was afterwards found by Mr. Strickland; and I have since seen a graptolithus which was discovered in them by Miss M. Lowe, and a minute Discina. Thus the Olenus shale of Malvern, is very analogous to the Alaunschiefer of Norway, one of the oldest of the Lower Palæozoic strata, and like that rests on fucoidal sandstone. Then followed an irruption of igneous rocks (*e*), which have burst through the syenite, the sandstone, and the shale, and now fill fissures in these rocks. The shale is bleached, and the sandstone is indurated, and otherwise altered by the dykes. The intrusive rocks of this era are either greenstone, or of a felspathic character. They are found in several small mounds, above the black shales, in a little crescent on the west side of the Malvern chain, and must be regarded as quite distinct in geological age and mineral constitution from the ordinary and more ancient plutonic rock of the hills. Such cases of greenstone dykes, formed at a later period than granitic and syenitic rocks in the same region, occur elsewhere; and show that, under a given surface, fused rocks of different quality have been flowing at different times—the trisilicated compounds being oldest.

Conglomerate beds, containing small fragments of syenite, masses of felspar, and quartz, and sandstones of gray and purple hues succeed (*f*), and are traced both at the north and south ends of the chain, but not in the middle parts; which perhaps might then be partially above water, so as to cause local unconformity. These strata, which are about 600 feet thick, are quite vertical at the north end of the ridge, and but moderately inclined in the southern part, where they form a bold ridge beyond the crescent of the bosses of trap. The organic remains found in these strata are not numerous, except towards the upper part, near the obelisk, in Eastnor Park. They are on the whole more allied to Upper Silurian than to Lower Silurian forms, but include besides some peculiar species, as Arca Eastnori, Lingula crumena, L. attenuata, and several Nuculæ. Trilobites are very rare in this group of strata. The strata next succeeding, are usually sandstones of finer grain, in thinner laminæ, alternating with sandy shales, to which as a really distinct member of the series, I gave the name of "*Upper Carodoc.*" It is now called Mayhill sandstone (*g*).

In the upper part, the hard beds are occasionally subcalcareous, and thus indicate the passage to the Woolhope limestone series; in *the lower part*, perhaps at the very boundary, is a remarkable very

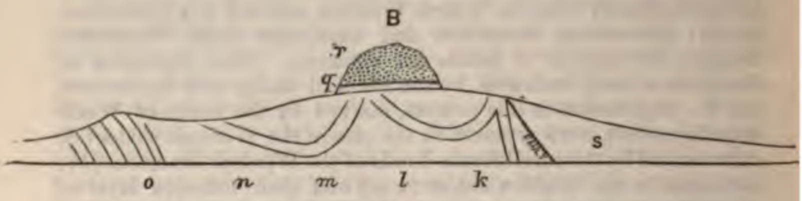
limited band, containing fragments of the syenite, mixed with many shells and corals, and some trilobites. This is seen in contact with the syenite at the western foot of the Worcester Beacon, in vertical strata, ripple-marked on the surface, and perfectly free from any metamorphic effect. It is somewhat surprising to find such delicate encrinites as hypanthocrinus and dimerocrinus, lying with rhynconellæ, favosites, petraiæ, and palæchinus, mixed with masses of syenite and felspar, mostly angular in shape, and evidently accumulated near the places where they had first become detached, the whole stratified, and the stratification vertical. This remarkable deposit was discovered by a lady resident at Malvern, in 1842.

During the deposition of the Mayhill group of laminated sandstones and shales, a downward movement of the whole sea-bed must have taken place; but, towards the close of this period, the descent must have become very slow, to allow for the almost purely calcareous, very shelly, and often coralliferous Woolhope limestone (*h*). The most interesting locality where this appears at the surface is in the little glen which descends westward from the north side of the Worcester Beacon, and passes by the shelly conglomerate bands above mentioned. In this glen, though now much obscured, the Woolhope limestone may be seen in two principal bands alternately with the Mayhill sandstones, and all the beds overthrown, beyond the vertical, so as to appear to dip inwards towards the chain. For the first notice of this remarkable fact we are indebted to Mr. L. Horner, the first accurate explorer of this region. This phenomenon of inverted strata, as they are called, appears at many points to the northward; and is well exhibited in the west flank of the Abberley Hill, in Upper Silurian, and old red formations. In the alternating sandstones and limestones under Worcester Beacon, the groups of fossils are different. The depression of the strata already indicated by the Mayhill shales and sandstones, and the retardation in this process, marked by the bands of Woolhope limestone, were repeated in the case of the Wenlock shales (*i*), surmounted by the coralliferous bands of the Wenlock limestone (*k*); and again in the Ludlow shales (*l*, *n*,) and their included layer of Aymestry limestone (*m*), which in the Malvern district, are of less importance than in some other tracts. At the very top of the Ludlow series, we have the first positive indication of neighbouring land in the portions of plants—small carbonaceous masses,—which occur at Stoke Edith, on the borders of Woolhope Forest; and on the same horizon, and for a small depth below, occur the earliest traces of fishes. In this upper part of the Silurians, sandy layers prevail more than in any other part of the series above the Wenlock limestone. The Downton sandstone, which occurs in this situation, offers an easy transition to the old red sandstones and marls, rather than shales, with cornstone (*o*). The depression of the sea bed continuing, alternate deposits of sandstone and marls, capped by conglomerate, prevailed for several thousand feet.

At this point of geological time, the surface of the syenite, where it is now covered by Hollybush sandstone, may very probably have been sunk to the depth of at least 8,000 feet.*

In a tract of country lying not far to the south of Malvern, and stretching through the South Wales coalfield, westward to and beyond the extremity of Ireland, and eastward through Belgium and across the Rhine, the depression of the sea-bed continued; and the whole series of carboniferous deposits, in the sea and at the border of the sea, to the depth of three miles, was accumulated. There is *no evidence* that this took place on the lines of Mayhill, the Malvern and Abberley hills; rather, by some *want of conformity* of the coal to the old red at Newent, on the line of boundary between old and new red from Newent to Haffield, and again north of Malvern, and along the Abberley hills,—it appears that the old red and Silurian strata *on this tract* had been uplifted, folded, and even wasted by surface action, before the date of the coal strata. Thus may be explained the absence of mountain limestone, for so great a space along the tract, of which Malvern is the centre, and probably the same arguments apply to the very limited exhibition of coal deposits on this line. Such deposits of limestone and of coal may have existed farther west, where the depression perhaps continued, and been removed by surface waste; and they may still exist farther east under the vale of the Severn, covered up and protected by later deposits.

ABBERLEY HILLS.



h, l, m, n, Silurian strata folded and worn away at top by surface action, prior to deposition of *q*, coal, and *r*, Permian conglomerate. At *o*, Old red and upper Ludlow are seen *overthrown*. The fault depresses the strata on the eastern side, where triassic beds, (*s*) occur.

From this mere sketch of a great subject, it appears that the upward and downward movements of ground, which in the volcanic region of Italy, and the Palæozoic tracts of Scandinavia, affect us with a lively interest, are the modern differentials, by which to integrate the far grander phenomena of the unstable earth-crust of

* The thickness of the old red sandstone being above 4000 feet in the country west of Malvern, and that of the Silurians, somewhat less than 4000.

earlier periods of time.* Perhaps no geological phenomenon is more certain or more significant of the true condition of the earth's mass than this repeated upward and downward movement of the crust in one region—this contemporaneous rising of one tract, and sinking of another not far removed—this wasting of one palæozoic shore, while another neighbouring palæozoic basin was receiving additional sediments. Dry and elevated, while to the northward and southward the sea was rich with the thousands of organic forms of the carboniferous æra, the ridges of Malvern appear to have never since been covered by the deposits from water, and may thus claim to be regarded as a tract of the most ancient land in Britain, composed of some of its oldest strata, resting on one of the oldest of its pyrogenous formations.

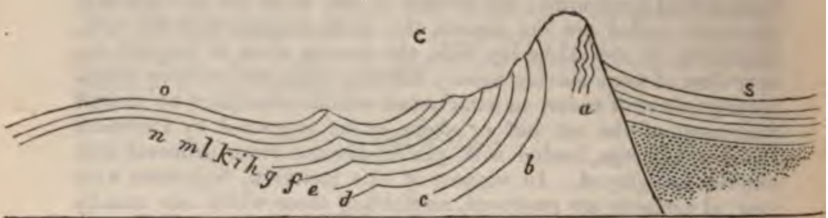
The very limited beds of coal in the Abberley hills are covered by a remarkable deposit of the Permian age, full of large and small masses of rock, derived partly from the neighbouring hills, and partly from points at a greater distance,—a conglomerate or a breccia, according as the fragments are rolled, subangular, or angular. The course taken by these fragments appears to be from the northward; under the influence of littoral currents they have been drifted southward; the farther to the south (as at Haffield), the more rolled are the masses, the more conglomeritic the rock. Examined in the Abberley hills, the masses seem to require the operation of some moving agency different from the ordinary transporting power of water—a cataclysmal action, consequent on violent movement of the sea bed or shore, or, as suggested by Professor Ramsay, icebergs, loaded with glacial detritus from primeval hills like the Longmynd. In support of this last view, fragments with striated surfaces are produced, resembling those which are usually accepted as evidence of glacial deposits.

Thus a somewhat new view of the palæozoic state of this part of the north temperate zone arises for consideration, viz., the formation of glaciers in lat. 53°, after the growth of the coal plants, and the formation of the coral reefs of the mountain limestone. Striation of the surface of stones is, however, an effect not confined to glacial movements: it occurs often along surfaces of dislocation, and may frequently, under the name of "slickenside," be detected running in different directions along very many joint faces where the movement of the rocks has been impeded, and the parts of the rock have been readjusted. I found in the great quarry at North Malvern a good example of this head, on the line of the fault which there cuts off the Syenite, and cuts through these very Permians. Following the principal fault lines in this quarry, which are very conspicuous, we remark the universal scraping of the highly inclined surfaces in lines parallel to the movement, and observe the crush-

* Considerations of this kind are familiar to the readers of the works of Sir Charles Lyell.

ings of the feeble parts, and the flutings and striations of the harder masses—a phenomenon also exhibited in the new railway tunnel in many examples.

The great fault to which attention has thus been called, as the last great line of movement traceable in the Malvern district, appears to have affected the strata before the Mesozoic age began —there is no trace of new red deposits on the west of it. There are, however, reasons for thinking that movements on the line of it were continued into the period of these deposits. This has lately been put in strong evidence by the progress of the railway tunnel between Great Malvern and Malvern Wells. Here the line of the fault has been crossed, and found to be (as indeed it also appears to be at North Malvern) complicated by many fissures, and much movement among the displaced masses of hard rock on which "slickensides" abound; the triassic beds being reared against it as much as 30° and 40°, a greater angle than my observations in 1842-4 led me to expect, but leading to the same conclusion: viz., that the last great movement on the line of fracture extending from the district of Tortworth, by Mayhill, Malvern, and Abberley, was



not completed till the Mesozoic ages had begun. Thus, then, finally, the section across the Malvern Hills shows movements having the characters sketched in the diagram C, in which it may be remarked that the folds of the strata seen on the west of the Malvern, are broad and gentle at a distance from the hills, but sharper and steeper, and even reversed in dip, near to the syenite. This is a local example of a general truth, some time since specially indicated along the Alleghanies, by Professor Rogers. The curvature of the beds in the Malvern district is so great, that the *horizontal* extent now occupied by them is less by one-fourth or one-third of a mile than the extent measured along the curvature—an indication that in these foldings of the strata much lateral compression has occurred. If we suppose a limited basin in which the strata have descended by continual depressions one, two, or more miles, to be again elevated, a compression of the strata in the proportion of the arc to the chord must be the result—lateral pressure *as an effect of* vertical movement—foldings to adjust the length of

the arc to the length of the chord—these being more violent towards the sides of the basin. Thus, in the central part of the great basin of South Wales, the folds are few and very broad; but on the north the Vale of Towy, and on the south the Cliffs of North Devon, are traversed by many very steep and complicated undulations of the strata, which *seem* to be axes of violent movement, and yet are, perhaps, *really* the result of gradual lateral thrusts occasioned by vertical movements on parallel lines at some distance—movements which it is the business of a general theory to explain.

The phenomena of the succession of life in the Malvern district were also treated of in this discourse, and illustrated by diagrams representing the increase of *variety* of specific and generic forms (not of the *number of individuals*), in different geological ages; the summary being to the effect that *a curve of life* may be drawn for the north temperate zone, the ordinates of which are reciprocals of the abscissæ, or in other words, the variety of the forms of life, beginning from zero in the hypozoic strata augments with the lapse of time, toward the most recent strata. The curve has, however, several points of contrary flexure, the most remarkable being two, viz., first, at the close of the Palæozoic period; and secondly, at the close of the Mesozoic series; at which times also occurred very great and general changes of physical geography, accompanied, no doubt, by the drying of some oceanic basins, and the extinction of their peculiar forms of life, and the opening of others to seas in which new races had begun to take their place in the diversified system of nature.

[J. P.]

WEEKLY EVENING MEETING,

Friday, March 20.

His Grace the Duke of Northumberland, K.G. F.R.S.
President R.I. in the Chair.*

John Watkins Brett, Esq. M.R.I.

On the Submarine Telegraph.

I purpose this evening to give you a brief sketch of the part I have taken in the promotion of submarine telegraphs, and shall strictly confine myself to a narrative of this enterprise, and some of the difficulties I have had to encounter at different stages of their progress.

It has been stated by some that I had sought, or attempted to appropriate to myself, the honour of the invention, of the submarine telegraph. I will here state, that my first idea of submarine telegraphs arose out of a conversation with my brother early in 1845, when disoussing the system of electric telegraphs, as then recently established between London and Slough; and, in considering the practicability of an entire underground communication, the question arose between us, "If possible underground, why not under water?" and "If under water, why not along the bed of the ocean?" The possibility of a submarine telegraph then seized upon my mind with a positive conviction; and I was ignorant until three or four years since that a line across the Channel had been previously projected by that talented philosopher, Professor Wheatstone,† (who, it will be remembered, with Mr. Cooke, first introduced the electric telegraph into this country,) and also of the experiments by frictional electricity during the last century, to send a current across rivers.

It is now nearly twelve years since (June 1845) my brother and I entered, in our joint names, at the Government Registration Office, a project for uniting America with Europe, by the very route now adopted; and in July of the same year submitted to the Government a proposition for uniting our Colonies with Great Britain, offering to Sir Geo. Cockburn, First Lord of the Admiralty, (to whom I had been referred by Sir Robert Peel,) as a first

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* H.R.H. The Prince of Wales was present.

† The original plans of Professor Wheatsone's project of a Sub-marine Electric Telegraph between Dover and Calais, drawn in 1840, were exhibited in the Library.

experiment to place Dublin Castle in instantaneous communication with Downing Street, provided £20,000 was advanced by the State towards the expense. This offer not being accepted, I turned my attention to the Continent, which I visited, and spent large sums of money in endeavouring to promote the electric telegraph in France, Prussia, and other Continental States. In 1847, I succeeded in obtaining permission from Louis Philippe to unite England with France by a submarine line, but failed to obtain the attention of the public, it being considered too hazardous for their support.

When the course of events placed Louis Napoleon at the head of the French nation, I brought the subject under his notice, soliciting such protection as I thought would induce the public to support the undertaking, and received an encouraging reply; nevertheless, £2000 only was subscribed for the first experiment.

The first attempt to connect England and France by a submarine telegraph was made in 1850, with a copper wire inclosed in gutta-percha, a material which opportunely came to our aid about that time. About 27 miles of this wire were conveyed on board the *Goliath* steam-tug, and wound round a large iron cylinder or drum to facilitate the paying it out, and the vessel started from Dover, exciting little or no curiosity at the time. The end of the wire attached to land was carried into a horse-box at the South-Eastern Railway Terminus, and we commenced paying out the wire, pieces of lead being fastened to it at intervals to facilitate the sinking. Electric communication from the vessel to the shore being kept up hourly, during its progress, the only drawback was a fear lest this frail experimental thread should snap, and involve the undertaking in ridicule. The trial was, however, successful; and the *Times* of the day justly remarked, "the jest of yesterday has become the fact of to-day."

The place chosen on the French coast for landing the wire was Cape Grinez, under a cliff among rocks; being purposely selected, because it afforded no anchorage to vessels, and was difficult of approach.

My station at the Dover Railway afforded an elevated position, from whence, by the aid of a glass, I was able to distinguish the light-house and cliff at Cape Grinez. A declining sun enabled me to discern the moving shadow of the steamer's smoke on the white cliffs, thus indicating her progress. At length the shadow ceased to move. The vessel had evidently come to an anchor. We gave them half an hour to convey the end of the wire to shore, and attach the printing instrument; and then I sent the first electric message across the Channel—this was reserved for Louis Napoleon. I was afterwards informed that some French soldiers who saw the slip of printed paper running from the little telegraph instrument, bearing a message from England, enquired, "how it could possibly have crossed the Channel?" and when it was explained that it was

the electricity which passed along the wire, and performed the printing operation, they were still incredulous. After several other communications, the words "All well," and "Good night," were printed,* and closed the evening.

In attempting to resume communication early next morning, no response could be obtained; and it soon became evident that the insulation was destroyed, either by a leakage of the electric current, or by its having snapped asunder.

It was conjectured, by the indications of the galvanometer, that it had parted near the French coast, which fact was ascertained on the return of our steamer, when we fished up the end.

Knowing the incredulity expressed as to the success of the enterprise, and that it was important to establish the fact that telegraphic communication had taken place, I that night sent a trustworthy person to Cape Grinez, to procure the attestation of all who had witnessed the receipt of the messages there; and the document was signed by some ten persons, including an engineer of the French Government, who was present to watch the proceedings; this was forwarded to the Emperor of the French, and a year of grace for another trial was granted.

Thus encouraged, and aided by the support of friends, including the late Lord De Mauley, who ever rendered the most willing cooperation to the enterprise, a more permanent cable was submerged in September 1851, between the South Foreland and Sangate on the French coast; and the line of telegraphic communication between England and France, now exerting so great an influence on the interests of those countries, was established.

In May 1853, the Dover and Ostend line was laid down. This commenced at the South Foreland, and terminated at Middle Kirk, about five miles from Ostend. Its length was about 70 miles. Captain Washington, R.N., and Captain Smithett, rendered important service to this operation by directing our route. Up to this time the depths encountered had been, comparatively speaking, trifling, the British Channel nowhere exceeding thirty fathoms. My next trial was in the unknown depths of the Mediterranean. The same year concessions were granted to me by the French and Sardinian Governments, to unite the Islands of Corsica, Sardinia, and the colony of Algeria, with their respective capitals.

The cables were manufactured at Greenwich, and sent out in the steamer *Persian*, in July 1854. On arriving at Genoa, I found that the Sardinian government had placed three vessels of war at my disposal; and we set sail at ten at night, for the harbour of Spezzia, the place selected for making fast the cable; H.R.H. Prince Cariginan, several ministers, and the Ambassadors of England and France, accompanying us.

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* These communications were printed in Roman type.

The spot I had chosen, by accident, was a cliff under a building once occupied by the immortal Dante, and wherein he composed a portion of the *Inferno*. Here the end of the cable was attached amid a salute of 60 guns, a scene in striking contrast to our solitary proceedings at Dover, when the first cord that ever carried instantaneous intelligence from Continent to Continent was submerged.

Captain Marquis Ricci, a Sardinian naval officer of reputed skill, was on board, and advised us not to venture across the Mediterranean in a direct line, where we should have to encounter unknown soundings, but to make a curve of some miles by the Islands of Gorgonia and Capraia, where the depth would be little more than 100 fathoms. I replied, as greater depths would have to be encountered between Sardinia and Africa, it was better at once to prove the risk; and, accordingly, we proceeded in a direct line, accompanied by the frigate *Malfatano*, commanded by the Marquis Boyle, who rendered us valuable service, by directing our route, and by taking soundings as we progressed.

The cable was laid in coils in the hold of the vessel, and before paying out was passed four or five times round two large iron drums, running out over an iron saddle on the stern of the vessel to the water, the pressure at this point in deep water being very great. For 14 miles all went on steadily: the pressure now caused some of the wires of the outer covering to break from tension, and on entering the great depths, one of those sudden flights of the cable occurred, which has taken place once on each subsequent occasion between Sardinia and Africa; every means were employed to stop it, and after a few minutes its progress was arrested, and it was brought up by a dead stop, when it was found that the insulation was destroyed in a portion that had passed into the sea, during this violent flight. Our only chance now was to recover from the sea the injured portion, cut it out, and make a fresh join,—a difficult and tedious operation, which occupied upwards of 30 hours, the strain upon the cable being excessive, in consequence of the great depth, 230 fathoms. When the injured portion came up, it was found that the violent twist of the outer wire had cut into the gutta percha wire of the inner core, and exposed it to the fatal action of the water, which carried off the current. Having repaired the injury, an electric current was sent to Spezzia; and the soundness of the six conducting wires to land proving correct, we proceeded. On nearing the coast of Corsica in the evening a fire and flag on shore indicated the spot where the cable was to be made fast. The vessel anchored about a mile from the coast, and the remaining portion of the cable was conveyed ashore in boats. This done, an electric communication was instantly dispatched through the cable to Paris and Turin, announcing to the French and Sardinian Governments the telegraphic union of Corsica with France and Piedmont.

We then sailed for the Straits of Bonifaccio, and the submerging

of the submarine cable between Corsica and Sardinia was successfully accomplished a few days afterwards; also the completion of the land lines, in the aggregate about 500 miles in length, through the two islands, uniting the different towns. The possibility of establishing a line of telegraph through these wild and lawless districts had previously been questioned, a guard of horse being thought necessary to protect it from injury: but the result proved that the public in all parts of the world may be trusted; and it is remarkable that only one instance of wilful injury has ever occurred throughout these islands; and the line has been in daily use since April 1855, and the French and Sardinian Governments have forwarded several thousand messages annually through it.

The submarine cable for connecting Sardinia with Algeria was made the same year. It was 150 miles in length, and weighed 1200 tons, and, allowing for coals, required a steamer of 2000 tons to carry it. Being unable to procure a steamer of that size in this country, in consequence of the war with Russia, I applied to the Emperor of the French for one, and at the same time expressed a wish that the portion of the Mediterranean I was about to cross should be sounded. I was directed to call upon the Minister of Marine, the Emperor adding that he would speak to him on the subject. On seeing the Minister, however, and naming the size of the steamer required, I was informed that the Government had not a vessel of this size at their disposal. But the soundings were made, and the result proved depths of 3000 metres, or nearly two miles, being from fifty to sixty times the depth of the English Channel.

The impossibility of obtaining a vessel of the required size continuing, and my friends becoming impatient of delay, I engaged, the following year, the *Result*, a large sailing vessel, and two of the best steamers I could procure to tow her, and operations were commenced from Cagliari, in September 1855. We encountered, on entering the great depths, one of those alarming flights of the cable which occurred in the previous year. In this instance about two miles, weighing sixteen tons, flew out with the greatest violence in four or five minutes, flying round even when the drums were brought to a dead stop, creating the utmost alarm for the safety of the men in the hold, and for the vessel. It was brought up by its encircling a large timber in the hold. Our means on board proved ineffectual to raise it, and the capstan broke by its weight, and rough weather coming on the vessel became unmanageable, and lost her course; there was therefore no alternative but to sever it from the injured portion in the sea, and it being evident that a sailing vessel would not do, I resolved to cut the cable and save the eighty-six miles for another attempt.

I had another and longer cable manufactured for the ensuing year, and finding opinions strong against taking the deeper direct line, it was resolved to carry it east of Galita, making a detour round the island, and thence to La Calle on the African coast.

We started from Cape Spartivento on the 6th of August, 1856, and all went on most favourably for about 60 miles, our speed being 2½ nautical miles an hour, a considerable velocity for a cable, which, be it understood, has to be handed up by men below decks. At the repeated wish of the French authorities on board, who had been appointed by the French Government to direct our course, it was decided that our steamer should be towed; this I believe to have been a mistake, though, not being a sailor, I am perhaps not qualified to give an opinion upon the subject.

My attention on board was chiefly directed to the arrangement of the machinery, and the speed of paying out the cable, and regulating its progress by the log, a duty requiring unremitting attention. When we had successfully accomplished this 60 miles, one of those sudden and alarming flights of the cable occurred, similar to the one which had happened once in each former year. Fortunately it was arrested without difficulty in less than three or four minutes, but it was discovered that, in arresting it, the insulation of a portion which had run out some miles in the sea had been injured. The manner I proposed to the French commander to recover the injured portion of the cable was to sever it, and attach the sound end on board to his steamer, and by his endeavouring to maintain a fixed position while we steamed under it at half power until we came to the injured part, to repair it; but this was abandoned, as he was of opinion that his vessel, being of less power than ours, would only be dragged after us. I determined therefore to sever the cable, return to Sardinia, and raise it from that end. We proceeded to the island accordingly, and when near the shore fished up the cable, which was clearly discernible in the blue waters at a depth of ten fathoms, severed it, and passed one end fore and the other aft, giving each of them a turn round the drums, and then joined them over the deck of the vessel, and erected another wheel at the bows. This done, we steered forward under the cable, raising it from the depths forward and letting it back over the stern. This plan answered perfectly, and we recovered 18 miles from the sea, which it was calculated, with the 126 miles* on board, would be more than enough to reach Galita. When these 18 miles had been recovered, they were spliced on to the coil on board, then passed five times round each drum; and we lay to for the night.

On the morrow, at the suggestion of the French authorities, it was arranged that the speed should never be less than three nautical miles per hour; and this speed never was slackened, even when the log gave an increase to nearly 4 miles, during the whole time of the paying out of the 126 miles of cables, except on one occasion when the drums caught fire for a few minutes. With this exception it was all payed out with the most perfect success.

* Statute miles.

I placed three tried and brave men at the breaks, and had the result of the log placed before them every quarter of an hour, timing the revolution of delivery of the cable over the drums by a minute watch. These experienced hands nobly did their duty, and we never left our post by day or night. It was an anxious moment when at nightfall we were about to enter depths of 1600 fathoms, which exceeded by four times those thought to be practicable;* we had also during this part of the operation to change the continuation of the cable from the fore hold to that of the midships, or upper hold, and also to remove the mid deck, to enable the coil to come up from the lower deck, operations involving labour and great risk; yet we dared not stop, being warned that a perpendicular strain on the cable in great depths would be fatal.

In the morning Galita appeared in sight; onward we went, but did not appear to near it; an observation of the sun at the meridian proved that we were out of our course. We signalled to the commander of the French towing vessel, and gave our observation, which proved to be correct. The French commander attributed the deviation from his course to the currents, which he stated in his report took him 20 miles out of his course.

The distance to land now, according to our reckoning, was 12 or 13 nautical miles, whereas the length of telegraph cable remaining on board was only 12 statute miles. A consultation was therefore held with the French commander; and it was determined, that as it was impossible to reach land with the cable, and as we were in great depths to the west of our line of soundings, we should at once steer due east, to endeavour to recover the line of soundings and buoy the end of the cable in shallow water. I encouraged the exhausted men at the breaks, urging them not to give an inch more line than was necessary, that we might, if possible, reach shallow water. At length we came to the last mile, without a chance of reaching a shallow part. It now became necessary to prepare for eventualities, and it was decided we must endeavour to hold our position, in depths of 400 to 500 fathoms, for five or six days, while the *Tartare* went to Algiers for a barge or lighter, whereon to secure the end of the cable. We secured the cable along the side of the vessel with hempen fastenings, as a precaution against snapping, or being injured by friction, and restored the interior supports, removed from the vessel to allow the exit of the cable, as we were now left alone to the risk of rough weather. This, unfortunately, was not long in coming.

Two vessels only passed us during the six days. The first was a Newcastle collier, which we hailed, and as the master seemed indisposed to come to us, our captain put off to him in a heavy sea, and enquiring his destination, offered to send a telegraphic message

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* Four hundred fathoms having been said, by experienced engineers, to be *the greatest* depth practicable.

to his owners in England; to which the master replied by an incredulous grunt, remarking, "You haven't got a railway terminus on board." The other vessel gave us a friendly hail, enquiring our object in staying in such unfrequented waters, and the sight of a parasol on board cheered us with signs of civilisation. But to return to the cable. The storm continued, and on the third and fourth day the vessel plunged violently, causing great alarm for the safety of the cable; nevertheless, on the morrow, the fifth day we had hung on in hope, we received a telegraphic message from London *viâ* Paris, announcing the rapid progress of the additional length required, ordered by us through the telegraph four days previously. I was on the point of replying, when a sudden and violent plunge of the vessel caused me to exclaim, "It is gone;" and on trying the instrument, it proved there was no communication. We at once commenced hauling in the cable, and after some hours up came the end, broken, apparently by friction on the rocks at the bottom, about 502 fathoms in length from the vessel.

I must now briefly say a few words upon the Atlantic telegraph, and the great depths of the ocean. This subject would alone occupy an evening. I shall therefore only allude to a few general points in connection with it.

The great depths of the Atlantic have until within these very few years remained unknown. It has been stated that Sir John Ross sounded in the Pacific with a line of 10,000 fathoms, or about 11 miles, without touching bottom. Scientific men had also come to a conclusion that the greatest depths of the ocean did not exceed eight or nine miles, and the uncertainty as respects all former attempts at deep sea soundings, appears to have been very great, and in some cases it was supposed to be fathomless, eleven miles of wire having been cast out without touching bottom. At last it was proved that by a common twine thread, for a sounding line, and a cannon ball of 60 lbs. weight, for a sinker, attached, the line being allowed to run from the reel as fast as the ball would take it, (care being had to pay it out from a boat, which could be kept stationary to maintain the line as perpendicular as possible,) correct soundings could be obtained.

The essential point was to time each hundred fathom as it ran out; and by always using a line of the same size and a sinker of the same weight and shape, a law of descent was established by the time of descent, as it was found that 2 minutes and 21 seconds became the average descent from 400 to 500 fathoms, and

3 min.	26 secs.	1000 to 1100	fathoms
4 „	29 „	1800 to 1900	„

By this decreased ratio of descent it could be ascertained the moment the line touched the bottom, for when this took place the currents would sweep the line out, at a uniform rate, whereas the

cannon ball, while it continued to descend, would drag it out at a decreasing rate.

A contrivance was also made by which the ball would detach itself, quills being inserted at the end which passed through the hollow of the ball. Specimens of the bottom were thus brought up by the quills, on the ball striking the bottom, which proved to consist of the most minute microscopic shells.

The greatest depths which have been reached by these means in the North Atlantic Ocean have been 25,000 feet.

It was some two years since, when Mr. Faraday was explaining the subject of induction, that a fact was named to him of a current being obtained from a length of 300 miles of gutta-percha covered wire half an hour after contact with the battery. I remember speaking to him on the subject, and enquiring if he did not believe this difficulty was to be overcome, and I received from him every encouragement to hope it might; but it at once became necessary that this point should be cleared up, or it would be folly to pursue the subject of the union of America with this country by electricity. I at once earnestly urged on Mr. Whitehouse to take up this subject, and pursue it independently of every other experiment, and a successful result was at last arrived at on 1000 miles and upwards of a continuous line in the submarine wires in the several cables, when lying in the docks. It did not rest upon one but many thousand experiments; it was further proved on 2000 miles of subterranean wire in the presence of Professor Morse, while in this country, and beats from 230 to 270 per minute were recorded, or equal to twelve to fifteen words per minute.

In the Atlantic cable the copper or conducting wire is composed of seven twisted wires formed into one, thus avoiding the danger of a flaw, which any single wire might be subject to; three separate coats of gutta-percha are then laid one over the other, the wire being thus perfectly insulated. This gutta-percha core passes and repasses in the course of its manufacture no less a length than 40,000 miles. The outer iron wires, of which there are 126 to each mile, are twisted into strands each containing seven wires, making an aggregate of 315,000 miles to the 2500 miles of cable. The construction of the cable is under the control of Messrs. Bright and Whitehouse.

The ultimate union of America with Europe by electricity may now be considered a certainty. Providence has placed this object within our reach; there are no practical impossibilities in the way of its accomplishment; and those united with us in the undertaking do not regard the means required in comparison to the good to be accomplished.

[J. W. B.]

Mr. Faraday occupied a few minutes at the commencement of the evening in giving a brief account of Mr. C. V. Walker's mode of telegraphing electrically from one station on a railway to the next on either side; or from a break-down on the rail to either of these stations. Signal bells are arranged at such stations, each with its own battery; and the latter, being connected with the earth at one of their ends, are connected together at the other ends through the bells and the telegraph line; but so that the batteries are *opposed* to each other as to the currents they can produce: hence there are no currents, and the bells do not ring. But when the wire between the bells is connected with the rail or the earth, then both batteries can send forth their currents and both bells are rung. This necessary earth connexion can be made at one station, and then the bell rings at the next. If a break-down occurs, the guard connects the telegraph wire where he is with the rail or earth, and the stations on both sides of the accident are warned. Provision is made at some of the line posts, about a furlong apart, where the man, by touching a key, can at once give the notice required. The method was illustrated by experiments with the bells and wire arranged by Mr. Richard Knight.

Mr. Gassiot's great induction coil was also exhibited and referred to by Mr. Faraday, who described its particular points as to insulation, &c., and showed some of its fine results.

[M. F.]

WEEKLY EVENING MEETING,

Friday, March 27.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Robert Warington, Esq.

On the Aquarium.

The speaker opened the evening's demonstration, by stating that he had immediately responded to the invitation of the Managers of the Royal Institution to deliver this discourse, on what they had been pleased to call his "own subject," from the feeling, that as the originator of the aquarium, he was in duty bound to afford, to all those who had taken up this "new pleasure," every assistance, from the results of his own experience, that lay in his power, in order to render the undertaking more easy and pleasurable; and for this

purpose, he proposed to lay before his audience, as far as was practicable, a demonstration of the principles on which it was founded, particularly as very erroneous ideas had been promulgated on the subject, and instructions given in several most engaging publications, which might tend materially to mislead and disappoint those inclined to recreate themselves with this interesting subject.

History.—After a short sketch of the several discoveries in the various branches of science embraced in this subject:—as the experiments of Lower, Thurston, Hooke, and Mayow, on respiration and animal heat; the presence of air in water, and its necessity for supporting the life of fish, by the Hon. Robert Boyle; the discovery of fixed air, carbonic acid, by Dr. Black, and its production in respiration; the experiments of Priestley, Ingenhousz, and Sennebier, on the action of submersed aquatic vegetation exposed to light, in removing carbonic acid, and restoring oxygen to the air dissolved in water,—all of which had been since substantiated by numerous experimenters. A cursory review was then given of the common employment of the ordinary fish globe, the cisterns, tanks, pans, and tubs, with their fish and water plants, to be seen every day in our conservatories and green-houses, and the glass cylinders, used by almost every microscopist for preserving chara, nitella, vallisneria, and other like plants in which the circulation of the sap was visible; as also for propagating rotifers, stentors, and other microscopic animalcules; the consideration of which points brought the subject up to modern times. Mr. Warington then proceeded to give an account of his own experiments, and the reasons which had led to their commencement, namely, the statements made for a series of years in our works on chemistry,* that growing vegetation would counterbalance the vital functions of fish. To test the truth of this, and its permanence,† a large twelve gallon receiver was filled to about two-thirds its capacity with river water, and some clean washed sand and gravel, with several large fragments of rockwork placed in it, the latter so arranged as to afford shelter to the fish from the sun's rays. A good healthy plant of vallisneria spiralis was then transplanted, and as soon as it had recovered from this operation a pair of gold fish were introduced. The materials being thus arranged, all appeared to progress healthily for a short time, until circumstances occurred which indicated that another and very material agent was required to perfect the adjustment, and render it at all permanent, and which at the commencement of the experiment had not been foreseen. The circumstances alluded to arose from the natural decay of the leaves of the vallisneria, the increase of which rendered the water turbid, and caused a rapid growth of green confervoid mucus on the surface of the water, and upon the

* Brande's Elements of Chemistry, 1821, and repeated up to the present time.

† Quarterly Journal of the Chemical Society, 1850. Vol. iii. p. 52.

sides of the receiver; the fish also assumed a sickly appearance, and had this been allowed to progress they must have speedily perished. The removal of this decaying vegetation from the water as fast as it was formed, became, therefore, a point of paramount importance, and to effect this, recourse was had to a very useful little scavenger,—whose highly important and beneficial functions throughout all Nature have been too much overlooked, and its indispensable uses in the economy of animal life not well understood,—the water snail, whose natural food consists of decaying and confervoid vegetation. Five or six of these little creatures, the limnea stagnalis, were consequently introduced, and by their extraordinary voracity and continued and rapid locomotion, soon removed the cause of interference, and restored the whole to a healthy state.

Thus then was established that wondrous and admirable balance between the animal and vegetable kingdoms, and by a link so mean and insignificant as almost to have escaped observation in its most important functions. The principles which are here called into action are, that the water, holding atmospheric air in solution, is a healthy medium for the respiration of the fish, which thus converts the oxygen constituent into carbonic acid. The plant, by its vital functions, absorbs the carbonic acid, and appropriating and solidifying the carbon of the gaseous compound for the construction of its proper tissues, eliminates the oxygen ready again to sustain the health of the fish. While the slimy snail, finding its proper nutriment in the decomposing vegetation and confervoid mucus, by its voracity, prevents their accumulation, and by its vital powers, converts that which would otherwise act as a poisonous agent into a rich and fruitful pabulum for the vegetable growth. Reasoning from analogy, it was evident that the same balance should be capable of being permanently maintained in sea water, and thus a vast and unexplored field for investigation opened to the research of the naturalist; and this proved on trial to be the case.

Principles of the Aquarium: the Air contained dissolved in Water.—The ordinary atmospheric air is found to be composed of 79 volumes of nitrogen gas and 21 volumes of oxygen; and water has the power of absorbing gaseous bodies in varying proportions, thus:—100 volumes of water, at a temperature of 60° Fahr., and under ordinary barometric pressure, will absorb

1·56	volumes of	nitrogen gas,
3·70	„	oxygen gas,
100·00	„	carbonic acid gas,

and hence we find that the air absorbed by water, and existent in rivers to the extent of from 2 to 3 per cent., consists of about 29 of oxygen and 71 of nitrogen. In fresh fallen rain and melted snow, it ranges from 30 to 35 per cent. of oxygen, and in some spring

waters it has reached as high as 38 per cent. This oxygen, by the process of respiration, is converted into carbonic acid gas, or mephitic air, the choke damp of the coalpit, a gas highly poisonous to animal life; but here comes into play that beautiful and wonderful provision which, by the action of growing vegetation under the influence of the sun's light, converts this baneful agent into vital oxygen, the "breath of life."

Water, fresh and marine.—The water used for the aquarium should be clean, and taken direct from a river, or from a soft spring, and should not have been purified by means of lime.

As regards sea water, it should, if possible, be taken at a distance from shore, and at the period of high water. If artificial sea water is employed, it should be made either from the saline matter obtained by the evaporation of sea water,* or by the following formula:—

Sulphate of Magnesia . .	7½ oz.
" Lime	2¾ "
Chloride of Sodium . . .	43¼ "
" Magnesium . .	6 "
" Potassium . .	1¼ "
Bromide of Magnesium . .	21 grains.
Carbonate of Lime . . .	21 "

These quantities will make ten gallons. The specific gravity of sea water averages about 1·025; and when from evaporation it reaches above this, a little rain or distilled should be added, to restore it to the original density.

Vegetation.—The plants best fitted for fresh water are the vallisneria spiralis, the myriophyllum, ceratophyllum, and the anacharis, all of them submersed plants, and fulfilling the purposes required most admirably. From the great supply of food in the aquarium, the growth of the vallisneria is very rapid, and it requires, therefore, to be thinned by weeding; this should never be done until late in the spring, and on no account in the autumn, as it leaves the tank with a weakened vegetation at the very time that its healthy functions are most required.

The vegetation of the ocean is of a totally different character and composition, being very rich in nitrogenous constituents. There are three distinct coloured growths,—the brown or olive, the green, and the red. For the purposes of the aquarium, where shallow water subjects are to be kept, the best variety is the green, as the ulvæ, the enteromorpha, vaucheriæ, cladophora, &c. These should be in a healthy state, and attached to rock or shingle when introduced.

* This is prepared by Messrs. Brew and Schweitzer, of 71, East Street, Brighton.

We shall have occasion to notice the rhodosperms under the head of light.

Scavengers.—A most important element in establishing and maintaining the permanent balance between the animal and vegetable life; without which no healthy functions can be secured, and the aquarium must become a continued source of trouble, annoyance, and expense. The mollusc which was first employed, the limnea stagnalis, was found to be so voracious, as it increased in size, that it had to be replaced by smaller varieties of limneæ, by planorbis, and other species of freshwater snail. The number of these should be adjusted to the quantity of work they are required to perform. In the marine aquarium, the common perriwinkle fulfils the required duties most efficiently, and is generally pretty active in his movements. The varieties of trochus are also most admirable scavengers; but it must be borne in mind that they are accustomed to mild temperatures, and will not live long in a tank liable to much exposure to cold. The nassa reticulata not only feeds on the decaying matters exposed on the surface of the rockwork and shingle, but burrows below the sand and pebbles with the long proboscis erected in a vertical position, like the trunk of the elephant, when crossing a river. But in the ocean there are innumerable scavengers of a totally differing class, as the annelids, chitons, starfish, nudibranch molluscs, &c.; thus affording a most beautiful provision for the removal of decaying animal matter, and converting it into food for both fish and man.

Light.—It is most probable that the greater amount of failures with the aquarium have arisen from the want of a proper adjustment of this most important agent; the tendency being generally to afford as much sun's light as possible; but, on consideration, it will be found that this is an erroneous impression. When the rays of light strike the glassy surface of the water, the greater part of them are reflected, and those which permeate are refracted and twisted in various directions by the currents of the water; and where the depth is considerable it would be few rays which would penetrate to the bottom: but let the surface become ruffled by the passing wind, and it is little light that can be transmitted; and when this same disturbing causes lashes into waves and foam, not a ray can pass, and all below must be dark as night. Too much light should therefore be avoided; and the direct action of the sun prevented by means of blinds, stipling, or the like. It is a great desideratum to preserve the growth of the lovely red algæ in all their natural beauty, and prevent their becoming covered with a parasitic growth of green or brown coloured plants; this can be effected by modifying the light which illuminates the aquarium by the intervention of a blue medium, either of stained glass, of tinted varnish, coloured blinds, &c. The tint should be that of the deep sea, a blue free from pink, and having a tendency rather to a green hue. This modified light affects also the health of those creatures which are

confined to shallow waters, so that a selection of the inhabitants must be made.

Heat.—The proper control of this agent is also most material to the well-being of these tanks, for experience has proved that an increase or diminution of temperature beyond certain limits acts most fatally on many of the creatures usually kept. These limits appear to be from 45° to 75° Fahrenheit. The mean temperature of the ocean is estimated to be about 56°; and this does not vary more than 12° throughout the varying seasons of the year, showing the extreme limits to be from 44° to 68°. Great care should therefore be taken to afford as much protection as possible, by the arrangement of the rockwork, both from the sun's rays by day, and the effects of radiation at night, as from the small volume of water contained in the aquarium these effects are rapidly produced.

Food.—As many persons, to whom those interested in these matters have naturally looked for instruction, have decried the idea of feeding, it will be necessary to offer a few remarks on that point. How creatures, so voracious as most of the denizens of the water are, both fresh and marine, are to thrive without food, is a question it would be difficult to solve; common sense would say they must gradually decrease in size, and ultimately die from starvation. The food employed should be in accordance with the habits of the fish, &c. For the vegetable and mud feeders, vermicelli, crushed small, with now and then a little animal food, as worms, small shreds of meat, rasped boiled liver, and the like. For the marine creatures, raw meat dried in the sun and moistened when used, answers very well. Oyster, mussel, cockle, raw fish, shrimps, and the like matters may be employed; these should be cut or pulled into very small pieces, and never more given than they can at once appropriate; and if rejected by one it should be transferred to another, or removed from the tank. In the case of actinia, they require, from their fixed position, that the food should be guided to their tentacles; and if the animal food, of whatever kind, is soaked in a little water, and the water thus impregnated with animal fluids be dropped in moderate quantity into the tank it will afford food for the small entomostracha and smaller creatures with which the water abounds, and which constitute the food for many of them.

A few observations were also made on the construction of a microscope for the purpose of employment in connection with the aquarium, and the method in which such an instrument could be used.

[R. W.]

WEEKLY EVENING MEETING,

Friday, April 3.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Rev. J. Barlow, M.A. F.R.S. Vice-President & Sec. R.I.

On some Modifications of Woody Fibre and their Applications.

After all the soluble parts of a plant, its gum, its sugar, its extractive matter, and its aromatic oil, as well as its starch and gluten, have been separated, the residue is a substance to which the names of "lignine," "cellulose," "sclerogen," have been given. Of this substance vegetable fibre may be regarded as a natural modification. Having adverted to this fact, Mr. Barlow noticed the distinctive physical properties of fibre—its strength, its flexibility, its readiness (though to a certain extent elastic) to take a permanent set or bend. He adverted to an ingenious application which has been made of these qualities, while the fibres yet remain part of the wood in which they were formed. By powerful pressure, and the use of metallic bands during the process to support the wood, Mr. Blanchard,* of New York, succeeded in giving permanent curvature to beams and planks without injury to the fibre. The invention received the first class medal at the Paris Exhibition of 1855, and it is now adopted by the Timber-Bending Company. Specimens of wood, thus bent, were exhibited; and it was shown that the fibres evinced no tendency to straighten, unless exposed to the joint influence of heat and moisture.

But the principal subject of Mr. Barlow's discourse being the *Parchment-paper* invented and patented by Mr. W. E. Gaine, C.E., and about to be introduced into commerce by Messrs. Thos. De la Rue and Co., he confined his remarks principally to the physical and chemical properties of vegetable fibre when converted into paper. A sheet of unsized paper is the result of the same forces which produce the sand flagstones which pave our streets—the forces which cause particles, when brought together under water, to

* Rapports du Jury International (de l'Exposition de Paris 1855), Vol. i. p. 273.

remain in close contact after the water has been withdrawn. This was experimentally exhibited. It was also shown that, when the vegetable fibres were long and strong (as those of the *Daphne papyraica*, from which much of the Indian paper is made), the paper possesses the requisite strength. In other cases the paper is strengthened and rendered sufficiently impervious to fluids for all requisite purposes, by being made to imbibe, during the process of manufacture, vegetable or animal size. But all paper is liable to be disintegrated by water, however strongly it may have been sized.

The chemical composition and properties of woody fibre were then considered. The components of this substance are carbon, hydrogen, and oxygen; the last-named elements being combined in the same proportion as they exist in water. In this respect, woody fibre is identical with starch, dextrine, gum, and sugar. Unlike these substances, it is insoluble whether in water, ether, alcohol, or oil, and much more averse than they are to chemical change. Mr. Barlow called attention to the enormous inconvenience which would arise if water could dissolve cloth, or if vegetable tissues were easily decomposed. It is, however, many years since Braconnot* discovered that sawdust, linen, and cotton fabrics, &c. could be made to part with a portion of their constituent hydrogen in exchange for an oxide of nitrogen obtained from the decomposition of the nitric acid with which they were treated. Pelouze† afterwards applied this principle in operation on paper; and to the same principle must be ascribed the gun-cotton and collodion of Schönbein.‡ Taking what may be called the gun-paper (Pelouze's paper) as a type of all these substances, Mr. Barlow showed by experiment that it is inflammable and highly electrical, and that in consequence of the substitution of a certain number of equivalents (varying from five to three) of hyponitric acid (NO_4) for an equal proportion of hydrogen, it becomes 50 per cent. heavier than the paper out of which it was converted. Gun-cotton is soluble in ether and potash: the latter solution has the property of reducing silver, in a bright metallic mirror, from the nitrate of that metal.

The surface-action of vegetable fibre in receiving dyes was then mentioned, in order to introduce some researches recently made by M. Kuhlmann, Director of the Mint at Lille.§ Led to the investigation by the general notion that azotized substances, as wool, silk, &c. are more susceptible of dyes than are vegetable textures, M. Kuhlmann instituted a series of experiments on gun-cotton, both woven and in the wool, by which he discovered that

* Annales de Chimie, Vol. lii. p. 290. (1833.)
† Comptes Rendus, tome xxiii. p. 809. (1846.)
‡ Philosophical Magazine, Vol. xxxi. p. 7 (1847): and Athenæum for 1847, p. 100.
§ Études sur la fixation des couleurs dans la Teinture; Comptes Rendus, tome xlii. p. 673. (1856.)

cotton or flax, thus azotized, will not take dye; but that if either by spontaneous, or else by artificially-produced decomposition, the fibre loses part of its nitrous principles, it then actually combines with colours much more energetically than it did while in its natural state. Specimens of the cloth, which M. Kuhlmann had experimented upon, and which that gentleman had sent for illustration of this subject, were exhibited.

Having reminded the audience that, in all these cases, a change in chemical constitution accompanied the change in physical properties, Mr. Barlow contrasted with the pyroxylised textures of Kuhlmann and the gun paper of Pelouze, the woven fabrics subjected to Mercer's process, and the *Parchment-paper*, the invention of Mr. Gaine. By acting on cloth with chloride of zinc, tin, or calcium, with sulphuric and arsenic acid, and, especially, by the caustic alkalis in the cold (the temperature sometimes being lowered to $-10°$ Fahr.), Mr. Mercer has obtained many important effects on the fineness and the general appearance of cloth, and its susceptibility of dye. This subject was brought before the Royal Institution by Dr. Lyon Playfair, C.B.,* and it has since been closely investigated by Dr. Gladstone.† Mr. Mercer also experimented on the effect of acids on paper. It being known that sulphuric acid, under certain conditions, modified vegetable fibre, Mr. Gaine instituted a course of experiments to ascertain the exact strength of acid which would produce that effect on paper which he sought, as well as the time during which the paper should be subjected to its action. He succeeded in discovering, that when paper is exposed to a mixture of two parts of concentrated sulphuric acid (*s. g.* 1·854, or thereabouts) with one part of water, for no longer time than is taken up in drawing it through the acid, it is immediately converted into a strong, tough, skin-like material. All traces of the sulphuric acid must be instantly removed by careful washing in water. If the strength of the acid much exceeds or falls short of these limits, the paper is either charred, or else converted into dextrine. The same conversion into dextrine also ensues, if the paper be allowed to remain for many minutes in the sulphuric acid after the change in its texture has been effected.

In a little more then than a second of time, a piece of porous and feeble unsized paper is thus converted into the *Parchment-paper*, a substance so strong, that a ring seven-eighths of an inch in width, and weighing no more than 23 grains, sustained 92 lbs.; a strip of parchment of the same dimensions supporting about 56 lbs. Though, like animal parchment, it absorbs water, water does not percolate through it. Though paper contracts in dimensions by this process of conversion into *Parchment-paper*, it receives no appreciable increase of weight, thus demonstrating that no sulphuric acid is

* Proceedings of the Royal Institution, Vol. i. p. 134. (1852.)
† Journal of the Chemical Society, Vol. v. p. 17. (1853.)

either mechanically retained by it, or chemically combined with it. It has also been ascertained by analysis, that no trace of sulphur exists in the *Parchment-paper*. The fact of this paper retaining its chemical identity, constitutes an important distinction between it and the gun-papers of Pelouze and others. Unlike those substances, it is neither an electric, nor more combustible than unconverted paper of equal size and weight, nor soluble in ether or potash. Unlike common paper, it is not disintegrated by water; unlike common parchment, it is not decomposed by heat and moisture. In this remarkable operation, the action of the sulphuric acid may be classed among the phenomena ascribed to catalysis (or contact action). It is, however, conceivable that this acid does, at first, combine with the woody fibre, with or without the elimination of oxygen and hydrogen, as water; and that this compound is subsequently decomposed by the action of water, in mass, during the washing process, the sulphuric acid being again replaced by an equivalent of water; for, as has been before stated, the weight of the paper remains the same before and after its conversion. Mr. Warren De la Rue and Dr. Müller are engaged in researches on this subject, which will be hereafter published.

Those who are interested in chemical inquiry, will recal many instances of physical changes occurring in compound bodies, while these bodies retain the same elements in the same relative weights. The red iodide of mercury is readily converted, by heat, into its yellow modification; yet, by the mere act of being rubbed, it is made to resume its former colour. Nothing is added to or taken from this substance in the course of these changes. The inert and permanent crystals of cyanuric acid are resolved by heat into cyanic acid—a volatile liquid, characterised by its pungent and penetrating odour, and so unstable that, soon after its preparation, it changes into a substance (cyamelide) which is solid, amorphous, and destitute of all acid properties. These substances, as well as fulminic acid, (which, however, is known in combination only,) contain carbon, nitrogen, oxygen, and hydrogen, in the same relative proportion. But the closest analogy to the production of *Parchment-paper*, scientifically considered, is perhaps, afforded by what is called "the continuous process" in etherification. It will be remembered that, in this process, sulphuric acid, at a temperature of 284° Fahr. converts an unlimited quantity of alcohol into ether and water. In the first stage of this process, as explained by Williamson, it would appear that the sulphuric acid combines with the elements of ether to form sulphovinic acid; and that, in the further progress of the operation, this compound, by coming into contact with a fresh equivalent of alcohol, is, in its turn, decomposed, and resolved into ether and sulphuric acid. The ether distils over together with the water resulting from the decomposition of the alcohol: the sulphuric acid remains in the retort, ready to act on the next portion. Here, as in the case of the *Parchment-paper*, the

sulphuric acid does not form a permanent constituent of the resulting substance, though it takes so important a share in its production.

The strength of this new substance, before alluded to, and its indestructibility by water, indicate many uses to which it may be applied. It will, probably, replace, to some extent, vellum in book-binding; it will furnish material for legal documents, such as policies of insurance, scrip certificates, &c.; it will take the place of ordinary paper in school-books, and other books exposed to constant wear. Paper, after having been printed either from the surface or in intaglio, is still capable of conversion, by Mr. Gaine's method; no part of the printed matter being obliterated by the process. *Parchment-paper* also promises to be of value for photographic purposes,* and also for artistic uses, in consequence of the manner in which it bears both oil and water-colour.

[J. B.]

GENERAL MONTHLY MEETING,

Monday, April 6.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

John Alger, Esq.
William Bowman, Esq. F.R.C.S. F.R.S.
Francis H. Dickinson, Esq.
Alexander Guthrie, Esq.
Edmund Packe, Esq.
Manuel Perez, Esq.
José Maria Perez, Esq. and
John Webb, Esq.

were duly *elected* Members of the Royal Institution.

M. Henri Ste-Claire Deville, of Paris,

was unanimously elected an Honorary Member of the Royal Institution.

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* Photographs on this paper were exhibited.

Edward Cotton, Esq.
Rev. Robert Everest.
Thomas Williams Helps, Esq.
Dr. Alphonse Normandy, and
Richard Henry S. Vyvyan, Esq.

were *admitted* Members of the Royal Institution.

The special thanks of the Members were returned to William Richard Hamilton, Esq. F.R.S. M.R.I. formerly Treasurer R.I. for his munificent present of a copy of the great work of Lepsius, Denkmäler aus Ægypten und Æthiopien.

The Secretary announced that the following Arrangements had been made for the Lectures after Easter:—

Eight Lectures on Italian Literature, by James Philip Lacaita, Esq. LL.D.

Eight Lectures on Sound, and some associated Phenomena, by John Tyndall, Esq. F.R.S. Professor of Natural Philosophy, R.I.

Seven Lectures on the Relations of Chemistry to Graphic and Plastic Art, by E. Frankland, F.R.S. Professor of Chemistry in Owen's College, Manchester.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same.

From—

Actuaries, Institute of—Assurance Magazine, No. 27. 8vo. 1857.
Asiatic Society of Bengal—Journal, No. 259. 8vo. 1855.
Astronomical Society, Royal—Monthly Notices, Vol. XVII. No. 4. 8vo. 1857.
Barlow, Rev. John, M.A. F.R.S. V.P. and Sec. R.I. (the Author)—Christian Restraint and Christian Love: Two Sermons preached at Kensington Palace Chapel. 8vo. 1857.
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Boosey, Messrs. (the Publishers)—The Musical World for March 1857. 4to.
British Architects, Royal Institute of—Proceedings in March 1857. 4to.
Daguin, M. P. A. (the Author)—Traité de Physique. 2 vols. 8vo. Paris, 1855–7.
East India Company, the Hon.—Astronomical Observations at Madras. 1843–52. 2 vols. 4to.
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Mars and Saturn, as seen at Madras with an Equatoreal (March 1854, and Nov. 1852). Plates.
Editors—The Medical Circular for March 1857. 8vo.
The Practical Mechanic's Journal for March 1857. 4to.
The Journal of Gas-Lighting for March 1857. 4to.
The Mechanic's Magazine for March 1857. 8vo.
The Athenæum for March 1857. 4to.
The Engineer for March 1857. fol.
The Artisan for Jan.-March 1857. 4to.

Civil Engineers, Institution of—Reports, March 1857. 8vo.
Everest, Rev. R. A.M. M.R.I. (*the Author*)—Statistical Details respecting the Republic of Lubeck. 8vo. 1857.
Faraday, Professor, D.C.L. F.R.S. &c.—Königliche Preussischen Akademie. Denkschriften 1855. 4to. Berichte, Dec. 1856.
Rapports du Jury Mixte International. (Exposition Universelle de Paris, 1855.) 2 vols. 4to. 1856.
Forbes, Sir John, M.D. D.C.L. F.R.S. M.R.I. (*the Author*)—Of Nature and Art in the Cure of Disease. 12mo. 1857.
Franklin Institute of Pennsylvania—Journal, Vol. XXXIII. No. 2. 8vo. 1856.
Geological Society—Journal, No. 49. 8vo. 1856.
Graham, George, Esq. (*Registrar-General*)—Report of the Registrar-General for March 1857. 8vo.
Hamilton, William R. Esq. F.R.S. F.S.A. M.R.I.—Denkmäler aus Ægypten und Æthiopien, von R. Lepsius. 11 vols. fol. Berlin, 1848–56.
Howley, Mrs.—Views and Ground-Plans of Lambeth Palace in 1828 and in 1830.
Lankester, Edwin, M.D. F.R.S. M.R.I. (*the Author*)—First Annual Report of the Medical Officer of Health of St. James's Westminster. 8vo. 1857.
Report on the Cholera Outbreak in St. James's Westminster, in 1854. 8vo. 1855.
Lewin, Malcolm, Esq. M.R.I. (*the Author*)—The Government of the East India Company, and its Monopolies. 8vo. 1857.
A Gazetteer of India. By E. Thornton. 8vo. 1857.
Middleton, Mr.—Economical Causes of Slavery in the United States, and Obstacles to Abolition, by a South Carolinian. 8vo. 1857.
Newton, Messrs.—London Journal (New Series), March 1857. 8vo.
Norton, James, Esq. Sen. (*the Author*)—Australian Essays on subjects Political, Moral and Religious. 4to. 1857.
Novello, Mr. (*the Publisher*)—The Musical Times, for March 1857. 4to.
Phillips, Charles, Esq. A.B. (*the Author*)—Vacation Thoughts on Capital Punishments. 8vo. 1857.
Photographic Society—Journal, No. 52. 8vo. 1856.
Royal Scottish Society of Arts—Transactions, Vol. 4. 8vo. 1852–5.
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Statistical Society—Journal, Vol. 20. Part 1. 8vo. 1857.
First Report of the Committee on Beneficent Institutions. 8vo. 1857.
Taylor, Rev. W. F.R.S. M.R.I.—Addison's Remarks on Italy. 16mo. 1718.
Webster, John, M.D. F.R.S. M.R.I.—Reports on Bethlem and Bridewell Hospitals, &c. for 1856. 8vo. 1857.
Wheatstone, Professor, F.R.S.—Letters on the Stereoscope. 8vo. 1856.
Vincent, B. Assist. Sec. R.I.—The Song of the Bell, and other Poems, translated from the German: by M. Montagu. 8vo. 1854.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, April 24.

Sir Benjamin C. Brodie, Bart., M.D. D.C.L. F.R.S. Vice-President, in the Chair.

Professor A. C. Ramsay, F.R.S.

On certain peculiarities of Climate during part of the Permian Epoch.

The subject was divided into two parts: 1st, The early geological history of the Longmynd and the neighbouring Lower Silurian rocks, between the Stiper Stones and Chirbury in Shropshire; and 2nd, The nature and glacial origin of the brecciated conglomerates of Worcestershire and part of South Staffordshire, that lie near the base of the Permian strata.

The Longmynd consists of a high tract of barren ground in Shropshire, formed of the Cambrian grits, conglomerates, and slates that lie beneath the Lower Silurian strata. They attain a height of about 1700 feet above the sea. The beds stand nearly *on end*, and measured across the strike appear to be about 14,000 feet thick. This appearance may, however, be deceptive, as it is possible they may be doubled over in large contortions, the tops of the curves having been removed by denudation. They have heretofore yielded no fossils except a doubtful trilobite, and the marks of annelides and fucoids. They are overlaid by an equal amount of Lower Silurian strata between the Stiper Stones and Chirbury.* These consist of Lingula flags, and Llandeilo slates and grits full of the ordinary fossils of the period, and are associated with bosses of eruptive greenstone and beds of felspathic trap and ashes. The slates have often a peculiar porcelanic and ribboned character imparted to them by the igneous rocks, and all the igneous phænomena of the district are of Lower Silurian date.

Certain strata, known as the *Pentamerus* beds or Upper Llandovery and May Hill sandstones, lie at the base of the Wenlock shale, quite unconformably on the Cambrian and Lower Silurian

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* First described in the Silurian System.—*Murchison.*

rocks. These rocks contain a peculiar suite of fossils, among which *Pentamerus oblongus* is conspicuous. They are frequently highly calcareous and conglomeratic, and mixed with the fossils contain pebbles of green and purple grit and slate, derived from the waste of the Longmynd rocks, on the upturned edges of which they rest. These Pentamerus beds form an ancient consolidated beach that surrounded an island of Cambrian and Lower Silurian rocks, at the commencement of the Upper Silurian epoch. Outliers of this old beach lie on the flats and slopes at the Bogmine and elsewhere, near the summits of the Lower Silurian hills west of the Stiper Stones; and it was shown that during the formation of the beach the island slowly sank and was gradually encased in Pentamerus beds, and these in their turn were buried beneath the Wenlock shale and Ludlow rocks, and probably also the old red sandstone. This part of the subject was illustrated by an account of the gradual submergence of the coral islands of the Pacific. The ancient island was thus not only submerged, but also shrouded beneath many thousand feet of newer strata. While the island stood high above the water the *Pentamerus* beach began to be formed, but as it slowly sank the beach crept inward and upward at least 800 feet, with a gentle slope, so that finally before complete submergence only the higher summits stood above the sea, surrounded by a continuation of the beach. The higher prolongation of the beach was thus shown to be of later date than the parts formed round the earlier margin of the island, and *the opposite ends of a continuous stratum may thus be of different ages.* This was illustrated by the fossils that the Pentamerus beds of the Longmynd contain. In Wales the Pentamerus beds have been divided by Mr. Aveline, of the Geological Survey, into two sets, the Lower and Upper Llandovery beds, each characterised by its own group of fossils, or by peculiarities of grouping. It is the upper part only that surrounds the Longmynd. At the foot of the Longmynd and Lower Silurian hills the Pentamerus beds among other fossils contain *Pentamerus oblongus* in great plenty, and also *P. lens*. The first is scarce in the Lower Llandovery rocks, and common in the Upper. With the second the reverse is usually the case. It is found in the (geographically) lower part of the beach above described, but in the higher geographical prolongation at the Bogmine it does not occur. *Strophomena pecten* is common to all the Silurian rocks in and below the Wenlock strata, but it is especially abundant in the Wenlock rocks, and is common in the Bogmine outlier. *Goniophora cymbæformis* is essentially an Upper Silurian species. It is not found in the Upper Pentamerus beds of the ordinary type, but occurs at the Bogmine, and ranges through the Wenlock and Ludlow rocks up to the tilestone, close below the base of the old red sandstone. The same is the case with *Bellerophon trilobatus*, also a Bogmine and tilestone species. A trilobite *Phacops Downingiæ*, not known in the ordinary Pentamerus beds,

occurs in the Bogmine outlier, and low in the Wenlock or Denbighshire grits.* Other instances of the same kind might be cited. An undescribed species of Pleurotomaria has been found at the Bogmine, and nowhere else. These facts show *that the assemblage of fossils in the inland and geographically higher part of the beach is more exclusively of an Upper Silurian type than the assemblage grouped in the geographically lower part of the same bed. Stratigraphically the bed was quite continuous, and yet its opposite ends are of somewhat different geological date.* This point, though not essential to, is intimately connected with, the proofs of a period of cold during the deposition of the Permian conglomeratic breccias or Rothliegendes, seeing that some of these higher Silurian fossils are contained in the fragments that enter into their composition, and it is therefore particularly insisted on.

How long the island of the Longmynd remained buried beneath several thousand feet of Upper Silurian rocks and old red sandstone is uncertain. It is, however, certain, that this covering was partly removed by denudation before the deposition of the upper coal measures, for in Shropshire, part of these rocks lie directly on the Cambrian strata, although Cambrian pebbles have not yet been detected in them. But in the Permian brecciated conglomerates of Worcestershire, many fragments, believed to be derived from the Longmynd and its neighbourhood, have been found. These breccias occur either themselves resting unconformably on the coal measures, or on older rocks, or else associated with Permian marles and sandstones that occupy like positions. These are found near Enville, at Wars Hill, and Stagbury Hill, where they lie on the coal measures; at Woodbury, one of the Abberley Hllls, where they rest on the Upper Silurian rocks; on Barrow Hill, on the coal measures and old red sandstone; at Howler's Heath, in the South Malvern region, on the Upper Silurian strata; and on the Clent and Lickey Hills, Frankley Beeches, and at Northfield the Permian rocks below the breccia rest on the South Staffordshire coalfield. They also occur at Church Hill, 5½ miles north-west of the Abberley Hills, where an outlier of breccia lies directly on the Coal measures of the Forest of Wyre. In all these places the brecciated stones are bedded in a hardened red marly paste. The stones which it contains are (with very rare exceptions) *not formed from the waste of the neighbouring rocks on which they lie*, but of fragments, many of them identical in composition and character with the Cambrian and Silurian beds of the Longmynd, and consist of pieces of quartz rock, greenstone, felspathic trap, felspathic ash, black slate, jasper, grey and purple sandstone, green sandy slate, ribboned altered slate, quartz conglomerate, and *Pentamerus conglomerate and limestone.* These are mixed with other foreign fragments; but those enumerated, always form by far the majority.

* Named on the authority of Mr. Salter.

They are of all sizes up to 2½ and 3 feet in diameter. The majority are small, like the stones of the Pleistocene drift. Their forms are always angular and subangular, their sides usually smoothed, and sometimes polished, and scratched in a manner identical with some of the stones of the modern moraines of the Alps, or of the *glacial drift* of the Pleistocene period that spreads over the north of Europe and America. The manner in which the blocks lie rudely bedded in the marly matrix also precisely corresponds to many of the ice-drifted deposits of the Pleistocene epoch. In England, judging from their outcrops, they now occupy an area of at least 500 square miles, chiefly concealed by overlying deposits. If lithological character be any guide, they have mostly been derived from the conglomerates of green, grey, and purple Cambrian grits of the Longmynd and from the Silurian quartz rocks, slates, felstones, felspathic ashes, greenstones, and Pentamerus beds between the Stiper stones and Chirbury. Neither the Malvern nor the Abberley Hills, nor the South Staffordshire country, nor any of the other districts where the breccias occur contain rocks at the surface similar to those from whence the breccias have derived their materials. It has been asserted that they may have been formed from the waste of rocks concealed beneath the neighbouring New red sandstone. This is, however, an improbable assumption, and in the outlier of Church Hill, *which is altogether surrounded by coal measures*, the rocks are of the same far transported character as in other localities. If other patches were formed from rocks concealed by the New red sandstone, this outlier, according to the same reasoning, might be expected to be formed from the waste of the surrounding coal measures, which is not the case. If then the blocks of stone thât form the breccias were derived from the Cambrian and Silurian rocks of the Longmynd, it is of importance to know how far they travelled. From the Longmynd region Church Hill is from 25 to 30 miles distant; Howler's Heath, at the south end of the Malvern Hills, from 40 to 50 miles distant; and the places where they occur near the South Staffordshire coalfield, from 35 to 45 miles distant; and it was shown that so many angular and subangular fragments, some of them 3 feet in diameter, and forming deposits in places 400 feet in thickness, could only have been transported by floating ice. At Northfield especially, many angular slabs of the Pentamerus beds of the Longmynd district were found, some of them 2 feet across, containing fossils of *the later age* of that deposit, and in the same Pentamerus rock, are enclosed fragments of the Cambrian green slates that were deposited in it when it formed a Silurian beach, as explained at the beginning of the lecture. *In no other part of England have the Pentamerus beds this character*; and the evidence is, therefore, in favour of the supposition that they were transported from the Longmynd. As no other agent that we know, except ice, transports so many large *angular* blocks to a distance, it was shown that the same transport-

ing agent must have been at work over large areas of Europe during the deposition of the Rothliegendes of the Permian period; and if we admit this kind of evidence for the Pleistocene drift, it is contended that the same kind of evidence of transportation from a distance, size, angularity, smoothing and scratched surfaces, should be admitted with regard to the stones and boulders of the Permian breccias.*

Proofs were also adduced to show that the internal heat of the earth has exerted no important climatal influence during any of the geological periods from Silurian times downwards; and a diagram exhibited illustrative of the analogies shown by the small development of molluscous life during the cold of the Permian and Pleistocene epochs, the last of which, as far as its fossil shells are concerned, may be considered but as a subdivision of the recent period.†

[A. C. R.]

ANNUAL MEETING,

Friday, May 1.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors was read and adopted.

A List of Books Presented accompanies the Report, amounting in number to 312 volumes, and making a total, with those purchased by the Managers and Patrons, of 1186 volumes (including Periodicals) added to the Library in the year.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to Professor Faraday, for their services to the Institution during the past year.

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* The Rothliegendes of Thuringia, in general appearance, closely resemble the Permian brecciated rocks of the Clent Hills (for example), and many of the blocks of granite in the Thuringian rocks have been derived from parent rocks, unknown in the neighbourhood where these conglomerates lie.

† This subject has, in some of its details, been treated more fully in the Geological Journal 1855, page 185.

The following Gentlemen were unanimously elected as Officers for the ensuing year :—

PRESIDENT—The Duke of Northumberland, K.G. F.R.S.
TREASURER—William Pole, Esq. M.A. F.R.S.
SECRETARY—Rev. John Barlow, M.A. F.R.S.

MANAGERS.

John George Appold, Esq. F.R.S.
Sir Benjamin Collins Brodie, Bart. D.C.L. F.R.S.
Benjamin Bond Cabbell, Esq. F.R.S. F.S.A.
Thomas Davidson, Esq.
Warren De la Rue, Esq. Ph.D. F.R.S.
George Dodd, Esq. F.S.A.
Sir Charles Fellows.
Sir H. Holland, Bt. M.D. F.R.S. F.G.S.
John Carrick Moore, Esq. M.A. F.R.S. F.G.S.
Frederick Pollock, Esq. M.A.
James Rennie, Esq. F.R.S.
Joseph William Thrupp, Esq.
John Webster, M.D. F.R.S.
The Lord Wensleydale.
Charles Wheatstone, Esq. F.R.S.

VISITORS.

Beriah Botfield, Esq. M.P. F.R.S. F.S.A.
John Charles Burgoyne, Esq.
John Robert F. Burnett, Esq.
Edmund Beckett Denison, Esq. M.A. Q.C.
Hugh W. Diamond, M.D. F.S.A.
C. Wentworth Dilke, jun. Esq.
Edward M. Foxhall, Esq.
John Hall Gladstone, Esq. Ph.D. F.R.S.
John Hicks, Esq.
Capt. Robert M. Laffan, R.E.
Thomas Lee, Esq.
Rev. Frederic D. Maurice, M.A.
Thomas N. R. Morson, Esq.
Joseph Skey, M.D.
Thomas Young, Esq.

WEEKLY EVENING MEETING,

Friday, May 1.

REV. J. BARLOW, M.A. F.R.S. Vice-President and Secretary, in the Chair.

CAPTAIN JOHN GRANT, late R.A.

*On the Application of Heat to Domestic Purposes and to Military Cookery.**

THE science of heat is more studied, and probably better understood in England than in any other country; but there is a

* The discourse was illustrated by models and diagrams of the cooking apparatus invented and described by Capt. Grant.

deficiency in the practical application of heat to domestic and other useful purposes, which deprives all classes of much comfort and benefit. For economy in the consumption of fuel is a matter of great importance to the mass.

Domestic Fire Places.—The universal practice of fixing grates, and surrounding them with masonry, is defective in principle; for the masonry not only absorbs a large portion of the heat which is required to warm the room, but throws a still larger portion up the chimney.

The heat which is thrown out from grates set in this manner does not depend so much upon the quantity of fuel they contain, as upon the amount of radiating and reflecting surfaces with which that fuel is surrounded. The highly polished steel surfaces which adorn the grates of those who can afford them reflect and radiate a very large amount of heat in proportion to the fuel they contain; but as the mass of the people can ill afford this costly mode of obtaining heat, the object should be to secure the maximum of heat with the minimum of cost.

The detached or portable grate, similar to the Brussels stove, will secure these advantages; and if properly constructed, combines all that can be desired for a domestic fireplace. It presents an open cheerful fire, easy to regulate,—affords a large radiating surface,—facilitates the operation of sweeping the chimney,—and is the best security against a smoky one; consequently dispenses with those countless varieties of infallible curatives, called chimney tops, which so disfigure all our houses. It acts also as a hot-air stove.

Domestic Cooking Apparatus.—The objection to casing our domestic grates with masonry applies still stronger to cooking ranges, for the wasteful expenditure of fuel is most apparent in all our cooking arrangements. The remedy here is equally simple, by the substitution of a detached cooking apparatus placed in the same recess which receives the ordinary fixed kitchen range; and a portable kitchen stove of this construction may be seen in daily operation at that interesting establishment, the North-West Reformatory Institution, in the New Road, where they are manufactured by the inmates. Eighty-two persons are daily cooked for at a cost of sixpence per day.

It may not be out of place to suggest to those who take an interest in the reformatory movement, a visit to that establishment, and see what has been accomplished through the exertions of one man (Mr. Bowyer), one of the most useful self-sacrificing philanthropists of the day.

The Cottager's Stove.—There is a large and important class of persons among the industrious and working classes, and also the indigent poor, whose domestic wants demand our consideration. For the purpose of at the same time warming their dwellings and cooking their food in a simple and economical manner, "*The*

Cottager's Stove" was designed, which has found its way to every quarter of the globe. It requires no fixing, is extremely simple in its construction, and all the operations of cooking may be carried on with any description of fuel. The fact of 100 lbs. of meat and 115 lbs. of vegetables having been cooked in one of these stoves, with less than 20 lbs. of coal, will suffice to prove how economically it may be worked, and it is peculiarly well suited to the rural districts. These stoves are manufactured by the Messrs. Bailey, of High Holborn.

Military Cookery.—The events of the late war have given rise to a variety of inventions and improvements, and amongst them that of cooking for large masses of troops, with convenience and economy, has been under consideration.

Field Cooking.—A system of field cooking was introduced at Aldershott, by cutting a trench in the ground and covering it with thin iron plates having a central hole in each to receive the ordinary camp kettle. A chimney is formed of sods, piled up to the height of three feet at one end of the trench, and a fire made at the other, and by this simple arrangement several regiments cooked for some months.

Battalion Cooking Apparatus for Troops, adopted at Aldershott.—This principle of cooking was adapted to the requirements of the troops in the hut barracks at Aldershott, and extended to the battalion cooking kitchens throughout the whole of that encampment, where it has continued in successful operation for the service of between 12,000 and 14,000 men, for these last twenty months. From April to August, in the last year, it was subjected to the severe test of cooking for 92,000 men, who marched in and out of the encampment during that period. The consumption of fuel requisite for this system of cooking is one half-pound of coal per man per day, and the official report states the cost to be one half-penny per man per week for the three daily meals.

Battalion Cooking Apparatus for Permanent Barracks, with Oven.—The soldier's cooking is confined to the boiling process; and as he can only obtain a change by availing himself of a public bakehouse at his own cost, an oven has been introduced into the chimney of the apparatus, so constructed that it is surrounded by the heated products of the two flues: thus profiting by what in ordinary cases is wasted up the chimney. 560° of heat were obtained by this arrangement in an oven capable of baking 250 lbs. of meat, without any additional consumption of fuel.*

* This principle of military cooking admits of so much variation in its details that it may be adapted to all the requirements of an army, either in the field or in permanent barracks, from a single soldier to a battalion of a thousand strong, for which provision is made.

The battalion cooking apparatus is constructed for that number of men, who may be efficiently cooked for by the aid of two small fires of 18 inches square

By the introduction of this oven, other improvements are effected: the flues are shortened, the fires brought nearer together, and the whole of the kettles boil simultaneously. The advantages of this system of cooking could only have been obtained by the application of the principle of horizontal draft; and the same principle may be carried out with equal advantage for warming the dwellings of the working classes. The subject is worthy the consideration of those interested in the philanthropic undertaking of erecting blocks of dwellings for our houseless poor. This principle will also be found applicable in many cases, where the perpendicular shaft is adopted in our large factories, pouring out its column of fire and wasted heat.

An apparatus, upon the principle of cooking for troops in permanent barracks, has lately been erected at the North-West Reformatory Institution, in the New Road, for the use of its inmates, which combines the means of cooking, baking, washing, drying the linen, and supplying hot water for baths. It might be extended with advantage to our prisons, unions, and all large establishments requiring such conveniences.

Public bakehouses might also avail themselves of this principle. By the aid of two small fires, heating the oven from without, and lining the interior with brick or tile, the baking might be carried on with some advantages over the present system.

[J. G.]

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and 6 inches deep, with any description of fuel, if the principle is properly carried out.

A school for the instruction of cooking might be added with advantage to the many excellent ones already established for the improvement and comfort of the British soldier.

GENERAL MONTHLY MEETING,

Monday, May 4.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

George Edward Dorington, Esq. and
Arthur Le Noè Walker, Esq.

were duly *elected* Members of the Royal Institution.

William Bowman, Esq. and
Major Lewis Burroughs

were *admitted* Members of the Royal Institution.

The following Professors were unanimously re-elected:—

WILLIAM THOMAS BRANDE, Esq. D.C.L. F.R.S. L. & E., as Honorary Professor of Chemistry in the Royal Institution.

JOHN TYNDALL, Esq. Ph.D. F.R.S. as Professor of Natural Philosophy in the Royal Institution.

The special thanks of the Members were voted to COL. SIR CHARLES HAMILTON, BART. M.R.I. for his present of Bouchette's Map of Lower Canada, in ten sheets.

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM—
Hon. East India Company—Report of Public Instruction in Lower Bengal, for 1855-6. 2 vols. 8vo. 1856.
Asiatic Society of Bengal—Journal, No. 258. 8vo. 1855.
Astronomical Society, Royal—Monthly Notices, Vol. XVII. No. 5. 8vo. 1857.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for April 1857. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for April 1857. 4to.
British Architects, Royal Institute of—Proceedings in April 1857. 4to.
Cambridge Philosophical Society—Transactions, Vol. IX. Part 4. 4to. 1857.
Chemical Society—Journal, No. 37. 8vo. 1857.

Editors—The Medical Circular for April 1857. 8vo.
The Practical Mechanic's Journal for April 1857. 4to.
The Journal of Gas-Lighting for April 1857. 4to.
The Mechanic's Magazine for April 1857. 8vo.
The Athenæum for April 1857. 4to.
The Engineer for April 1857. fol.
The Artisan for April 1857.
Faraday, Professor, D.C.L. F.R.S. &c.—Königliche Preussischen Akademie. Berichte, 1856, Jan. 1857.
Franklin Institute of Pennsylvania—Journal, Vol. XXXIII. No. 3. 8vo. 1856.
Geographical Society, Royal—Journal. Vol. XXVI. 8vo. 1857.
Geological Survey of India—Memoirs. Vol. I. Part 1. 8vo. Calcutta, 1856.
Graham, George, Esq. (Registrar-General)—Report of the Registrar-General for April 1857. 8vo.
Hamilton, Sir Charles, Bart. C.B. M.R.I.—Map of Lower Canada, in 10 sheets, by Joseph Bouchette.
Map of the Polar Regions, 1825; Chart of Greenland, with the N.W. Voyages of Hudson, Frobisher, and Davis. By A. Arrowsmith, 1825.
Charts of Islands on the Coast of Africa. 1829.
Linnean Society—Journal of Proceedings. No. 4. 8vo. 1857.
Londesborough, The Lord, K.H. F.R.S. M.R.I.—Miscellanea Graphica, Part 12. 4to. 1857.
Newton, Messrs.—London Journal (New Series), April 1857. 8vo.
Novello, Mr. (the Publisher)—The Musical Times, for April 1857. 4to.
Nutt, Mr. D.—Catalogue of Foreign Theological Books. 8vo. 1857.
Pearsall, Mr. Thomas—Catalogues des Bibliothèques du feu Roi Louis Philippe, de M. Tieck, &c. 8vo. 1852–5.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1857. Heft 1. 4to. Gotha, 1857.
Photographic Society—Journal, No. 53. 8vo. 1856.
Rammell, T. W. Esq. C.E. (the Author)—A New Plan for Street Railways. 8vo. 1857.
Royal Observatory, Greenwich—Astronomical, Magnetical, and Meteorological Observations in 1855. 4to. 1857.
Royal Society of Literature—Transactions, Vol. IV. and V. 8vo. 1849–56.
Royal Society of London—Proceedings, No. 25. 8vo. 1857.
Society of Arts—Journal for April 1857. 8vo.
Thomas, L. Esq. (the Author)—Rifled Ordnance. 8vo. 1857.
Williams, C. W. Esq. (through Jonathan Green, M.D. M.R.I.)—Prize Essay on the Prevention of the Smoke Nuisance, by C. W. Williams. 8vo. 1856.

WEEKLY EVENING MEETING,

Friday, May 8.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

F. CRACE CALVERT, ESQ. F.C.S. M.R.A. Turin,
HONORARY PROFESSOR OF CHEMISTRY, ROYAL INSTITUTION, MANCHESTER.

On M. Chevreul's Laws of Colour.

MR. CRACE CALVERT stated that he had three objects in view this discourse. The first was to make known the laws of colours, discovered by his learned master, M. Chevreul; secondly, to expl their importance in a scientific point of view; and, thirdly, th value to arts and manufactures.

To understand the laws of colours, it is necessary to know composition of light; Newton was the first person who gave to world any statement relative to the components of light, which said consisted of seven colours—red, orange, yellow, green, bl indigo, and violet. It is now distinctly proved that four of th seven colours of the spectrum are the result of the combinati of the three colours now known as the primitive colours, viz., r blue, and yellow. Thus blue and red combined produce purple indigo; blue and yellow, green; while red and yellow, prod orange: these facts being known, it is easy to prove that there not seven, but three primitive, and four secondary, called co plementary colours.

Several proofs can be given that light is composed of th colours only. One of the most simple consists in placing pieces blue, red, and yellow papers on a circular disc, and rotating rapidly; the effect to the eye being to produce a disc of wh light. If, therefore, the eye can be deceived so readily while t disc travels at so slow a rate, what must necessarily be the ca when it is remembered that light proceeds at the rate of 190,0 miles per second?

The rapidity with which light travels is such that the eye is r able to perceive either the blue, red, or yellow, the nerves of t retina not being sensitive enough to receive and convey successiv to the mind the three or seven colours of which the light is co posed.

Before entering into the laws of colour, Mr. Crace Calv stated that it might be interesting to know what scientific min had devoted attention to the laws of colours.

Buffon followed Newton, and his researches had special reference to what M. Chevreul had called the "successive contrasts" of colours.

Father Scherffer, a monk, also wrote on the laws of colour. Goethe, the poet, also brought his mind to bear upon the subject, and studied it to a great extent. Count Rumford, about the end of the eighteenth century, published several memoirs on the laws of colours. He explained very satisfactorily the "successive" contrast, and arrived at some insight into the "simultaneous" one; still he did not lay down its real laws.

Prieur, Leblanc, Harris, and Field, were also writers of most interesting works on this subject. The reason that they did not arrive at the definite laws of colour was because they had not divided those laws into successive, simultaneous, and mixed contrasts. These form the basis of the practical laws of colour, and the honour of their discovery is due to M. Chevreul.

The reason why a surface appears white or brilliant is, that a large portion of the light which falls on its surface is reflected on the retina, and in such a quantity as gives to the surface a brilliant aspect; whilst in plain white surfaces, the rays of light being diffused in all directions, and a small portion only arriving to the eye, the surface does not appear brilliant. The influence of colours on these two kinds of surfaces is very different, as may be perceived by the examples round the room, showing the influence of different colours on gold ornaments. When rays of light, instead of being reflected, are absorbed by a surface or substance it appears black; therefore white and black are not colours, as they are due to the reflection or absorption of undecomposed light. It is easy to understand why a surface appears blue; it is due to the property which the surface has to reflect only blue rays, whilst it absorbs the yellow and red rays; and if a certain portion of light is reflected with one of the coloured rays it will decrease its intensity; thus red rays with white ones produce pink. On the contrary, if a quantity of undecomposed light is absorbed, black is produced, which, by tarnishing the colour and making it appear darker, generates dark reds, blues, or yellows. The secondary colours are produced by one of the primitive colours being absorbed and the two others reflected; for example, if red be absorbed, and blue and yellow reflected, the surface appears green. There are two reasons why a perfect blue, yellow, red, cannot be seen, &c. The first is, that surfaces cannot entirely absorb one or two rays and reflect the others. The second is, that when the retina receives the impression of one colour, immediately its complementary colour is generated; thus, if a blue circle is placed on a perfectly grey surface, an orange hue will be perceived round it; if an orange circle, round it will be noticed a bluish tint; if a red circle, a green; if a greenish yellow circle, a violet; if an orange yellow circle, an indigo; and so on.

The "successive" contrast has long been known; and it consists in the fact that on looking stedfastly for a few minutes on a red surface fixed on a white sheet of paper, and then carrying the eye to another white sheet, there will be perceived on it not a red, but a *green* one; if green, *red;* if purple, *yellow;* if blue, *orange.*

The "simultaneous" contrast is the most interesting and useful to be acquainted with. When two coloured surfaces are in juxtaposition, they mutually influence each other,—favourably, if harmonising colours, or in a contrary manner if discordant; and in such proportion in either case as to be in exact ratio with the quantity of complementary colour which is generated in the eye: for example, if two half-sheets of plain tinted paper, one dark green, the other of a brilliant red, are placed side by side on a grey piece of cloth, the colours will be mutually improved in consequence of the green generated by the red surface adding itself to the green of the juxtaposed surface, thus increasing its intensity, the green in its turn augmenting the beauty of the red. This effect can easily be appreciated if two other pieces of paper of the same colours are placed at a short distance from the corresponding influenced ones, as below:—

Red. Red Green. Green.

It is not sufficient merely to place complementary colours side by side to produce harmony of colour, since the respective intensities have a most decided influence: thus pink and light green agree, red and dark green also; but light green and dark red, pink and dark green, do not; and thus to obtain the maximum of effect and perfect harmony the following colours must be placed side by side, taking into account their exact intensity of shade and tint.

Harmonising Colours.

Primitive Colours.	*Complementary Colours.*		
Red	Green	Light blue Yellow Red	White light.
Blue	Orange	Red Yellow Blue	White light.
Yellow-orange . .	Indigo	Blue Red Yellow	White light.
Greenish Yellow .	Violet	Red Blue Yellow	White light.
Black	White	Yellow Blue Red	White light.

If attention is not paid to the arrangement of colours according to the above diagram, instead of their mutually improving each other, they will, on the contrary, lose in beauty; thus if blue and purple are placed side by side, the blue throwing its complementary colour, orange, upon the purple, will give it a faded appearance; and the blue receiving the orange yellow of the purple will assume a greenish tinge. The same may be said of yellow and red, if placed in juxtaposition. The red, by throwing its complementary colour green, on the yellow, communicates to it a greenish tinge; the yellow, by throwing its purple hue, imparts to the red a disagreeable purple appearance. The very great importance of these principles to every one who intends to display or arrange coloured goods or fabrics was convincingly shown by Mr. Crace Calvert, from a great variety of embroidered silks (kindly lent by Mr. Henry Houldsworth), calicos, and paper-hangings, which demonstrated that if these laws are neglected, not only will the labour and talent expended by the manufacturer to produce on a given piece of goods the greatest effect possible, be neutralised, but perhaps lost. It was clearly demonstrated that these effects are not only produced by highly-coloured surfaces, but also by those whose colours are exceedingly pale, as, for example, light greens, or light blues with buffs, and that even in gray surfaces, as pencil drawings, the contrast of tone between two shades was distinctly visible. The contrast of tone or tint was most marked when two tints of the same colour were juxtaposed, and it was therefore the interest of an artist to pay attention to this principle when employing two tints of the same scale of colour. From the "mixed contrast" arises the rule that a brilliant colour should never be looked at for any length of time, if its true tint or brilliancy is to be appreciated; for if a piece of red cloth is looked at for a few minutes, green, its complementary colour, is generated in the eye, and adding itself to a portion of the red, produces black, which tarnishes the beauty of the red. This contrast explains, too, why the tone of a colour is modified, either favourably or otherwise, according to the colour which the eye has previously looked at. Favourably, when, for instance, the eye first looks to a yellow surface, and then to a purple one; and unfavourably, when it looks at a blue and then at a purple.

Mr. Crace Calvert also showed that black and white surfaces assume different hues according to the colours placed in juxtaposition with them; for example, black acquires an orange or purple tint if the colours placed beside it are blue or orange; but these effects can be overcome, in the case of these or any colours, by giving to the influenced colour a tint similar to that influencing it. Thus, to prevent black becoming orange by its contact with blue, it is merely necessary that the black should be blued, and in such proportion that the amount of blue will neutralize the orange thrown on it by influence, thus producing black. As an instance,

to prevent a grey design acquiring a pinkish shade through working it with green, give the grey a greenish hue, which, by neutralising the pink, will generate white light, and thus preserve the grey.

Mr. Crace Calvert, after explaining the chromatic table of M. Chevreul, which enabled any person at a glance to ascertain what was the complementary colour of any of the 13,480 colours which M. Chevreul had distinctly classed in his table, stated that it was of the highest importance to artists to be acquainted with these laws, in order to know at once the exact colour, shade, and tint, which would produce the greatest effect when placed beside another colour, and that they could save the great length of time which no doubt the great masters lost in ascertaining by experiment those laws, which they could now learn in a few hours by consulting M. Chevreul's work.

[F. C. C.]

WEEKLY EVENING MEETING,

Friday, May 15.

The Lord Wensleydale, Vice-President, in the Chair.

Thomas H. Huxley, F.R.S.

FULLERIAN PROFESSOR OF PHYSIOLOGY ROYAL INSTITUTION.

On the present state of Knowledge as to the Structure and Functions of Nerve.

The speaker commenced by directing the attention of the audience to an index, connected with a little apparatus upon the table, and vibrating backwards and forwards with great regularity. The cause of this motion was the heart of a frog (deprived of sensation though not of life) which had been carefully exposed by opening the pericardium, and into whose apex the point of a needle connected with the index had been thrust. Under these circumstances the heart would go on beating, with perfect regularity and full force, for hours; and as every pulsation caused the index to travel through a certain arc, the effect of any influences brought to bear upon the heart could be made perfectly obvious to every one present.

The frog's heart is a great hollow mass of muscle, consisting of three chambers, a ventricle and two auricles, the latter being separated from one another by a partition or septum. By the successive con-

traction of these chambers the blood is propelled in a certain direction; the auricles contracting force the blood into the ventricle; the ventricle then contracting drives the blood into the aortic bulb; and it is essential to the full efficiency of the heart as a circulatory organ, that all the muscular fibres of the auricles should contract together; and that all the muscular fibres of the ventricle should contract together; but that the latter should follow the former action after a certain interval.

The contractions of the muscles of the heart thus occur in a definite order, and exhibit a combination towards a certain end. They are rhythmical and purposive; and it becomes a question of extreme interest to ascertain, where lies the regulative power which governs their rhythm.

If we examine into the various structures of which the heart is composed, we find that the bulk of the organ is made up of striped muscular fibres, bound together as it were by connective tissue, and lined internally and externally by epithelium. Now it is certain that the regulative power is not to be found in any of these tissues. The two latter may, for the present purpose be regarded as unimportant, as they certainly take no share either in producing or guiding the movements of the heart. The muscular tissue, on the other hand, though the seat of the contractility of the organ, requires some influence from without, some stimulus, in order to contract at all, and having once contracted, it remains still until another stimulus excites it. There is, therefore, nothing in its muscular substance which can account for the constantly recurring rhythmical pulsations of the heart.

Experiments have been made, however, which clearly show that the regulative power is seated, not only in the heart itself, but in definite regions of the organ. Remove the heart from the body, and it still goes on beating; the source of the rhythm is therefore to be sought in itself. If the heart be halved by a longitudinal section, each half goes on beating; but if it be divided transversely, between the line of junction of the auricles with the ventricle and the apex of the latter, the detached apex pulsates no longer, while the other segment goes on beating as before. If the section be carried transversely through the auricles, both segments go on beating; and if the heart be cut into three portions by two transverse sections, one above the junction of the auricles and ventricle, and one below it, then the basal and middle segments will go on pulsating, while the apical segment is still. Clearly then, the source of the rhythmical action, the regulative power, is to be sought somewhere about the base of the auricles, and somewhere about the junction of the auricles and ventricles.

Now there is in the frog's heart, besides the three tissues which have been mentioned, a fourth, the nervous tissue. A ganglion is placed at the base of the heart, where the great veins enter the auricles—from this two cords can be traced traversing the auricular

septum, and entering two other ganglia placed close to the junction of the auricles with the ventricles. From these ganglia nerves are distributed to the muscular substance. Now we know, from evidence afforded by other striped muscles and nerves, that the contraction of the former is the result of the excitement of the latter; in like manner, we know that the ganglia are centres whence that excitement originates. We are therefore justified, analogically, in seeking for the sources of the contractions of the cardiac muscles, in the cardiac ganglia; and the experiments which have been detailed—by showing that the rhythmical contractions continue in any part of the heart which remains connected with these ganglia, while it ceases in any part cut off from them—prove that they really are the seats of the regulative power.

The speaker then exhibited another very remarkable experiment (first devised by Weber) which leads indirectly to the same conclusion. An electro-magnetic apparatus was so connected with the frog upon the table, that a series of shocks could be transmitted through the pneumogastric nerves. When this was done, it was seen that the index almost instantly stopped, and remained still, so long as the shocks were continued; on breaking contact, the heart remained at rest for a little time, then gave a feeble pulsation or two, and then resumed its full action. This experiment could be repeated at will, with invariably the same results; and it was most important to observe, that during the stoppage of the heart the index remained at the lowest point of its arc, a circumstance which, taken together with the distended state of the organ, showed that its stoppage was the result, not of tetanic contraction but of complete relaxation.

Filaments of the pneumogastric nerve can be traced down to the heart, and whenever these fibres are irritated the rhythmical action ceases. The pneumogastric nerves must act either directly upon the muscles of the heart, or indirectly through the ganglia, into which they can be traced. If the former alternative be adopted, then we must conceive the action of the pneumogastric nerve upon muscle to be the reverse of that of all other nerves—for irritation of every other muscular nerve causes activity and not paralysis of the muscle. Not only is this in the highest degree improbable, but it can be demonstrated to be untrue; for on irritating, mechanically, the surface of the heart brought to a standstill by irritation of the pneumogastrics, it at once contracts. The paralysing influence therefore is not exerted on the muscles, and as a consequence, we can only suppose that this "negative innervation," as it might be conveniently termed, is the result of the action of the pneumogastric on the ganglia.

It results from all these experiments, firstly, that nerve substance possesses the power of exciting and co-ordinating muscular actions; and secondly, that one portion of nervous matter is capable of controlling the action of another portion. In the case of the heart it

is perfectly clear that consciousness and volition are entirely excluded from any influence upon the action of the nervous matter, which must be regarded as a substance exhibiting certain phenomena, whose laws are as much a branch of physical inquiry as those presented by a magnet.

Now, (still carefully excluding the phenomena of consciousness,) we shall find on careful examination, that all the properties of Nerve are of the same order as those exhibited by the nervous substance of the heart. Every action is a muscular action, whose proximate cause is the activity of a nerve, and as the muscles of the heart are related to its ganglia, so are the muscles of the whole body related to that great ganglionic mass which constitutes the spinal marrow, and its continuation the medulla oblongata. This cranio-spinal nervous centre originates and co-ordinates the contractions of all the muscles of the body independently of consciousness, and there is every reason to believe that the organ of consciousness stands related to it as the pneumogastric is related to the cardiac ganglia; that volition whether it originates, or whether it controls action, exerts its influence not directly on the muscles but indirectly upon the cranio-spinal ganglia. A volition is a conscious conception, a desire; an act is the result of the automatic, unconscious origination and co-ordination, by the cranio-spinal ganglia, of the nervous influences required to produce certain muscular contractions.

Whatever may be the ultimate cause of our actions then, the proximate cause lies in nerve substance. The nervous system is a great piece of mechanism placed between the external world and our consciousness; through it objects affect us; through it we affect them; and it therefore becomes a matter of the highest interest to ascertain how far the properties and laws of action of nerve substance have been ascertained by the physiological philosopher.

Nerve substance has long been known to consist of two elements, fibres and ganglionic corpuscles. Nerve fibres are either sensory or motor, and the activity of any one fibre does not influence another. But when nerve fibres come into relation with ganglionic corpuscles, the excitement of a sensory nerve gives rise to that of a motor nerve, the ganglionic corpuscles acting in some way as the medium of communication. The "grey matter" which occupies the middle of the spinal marrow has long been known to be the locality in which the posterior roots, or sensory fibres, of the nerves of the body, and the anterior roots, or motor fibres, come into relation with ganglionic corpuscles; and as the channel by which, in what are called reflex actions, the activity of the sensory nerves is converted into excitement of corresponding motor nerves. The precise *modus operandi* of the grey matter has been much disputed, but the recent researches of Wagner, Bidder, Kupfer, and Owsjannikow, throw a great light upon, and vastly simplify the whole problem. It would appear that all nerve fibres are processes of ganglionic

corpuscles; that, in the spinal cord, the great mass of the grey matter is nothing but connective tissue, the true ganglionic corpuscles being comparatively few, and situated in the anterior horns of the grey substance; finally, it would seem that no ganglionic corpuscle has more than five processes; one, which becomes a sensory fibre and enters the posterior roots of the nerves; one, a motor fibre which enters the anterior roots; one, which passes upward to the brain: one, which crosses over to a ganglionic corpuscle in the other half of the cord; and perhaps one establishing a connexion with a ganglionic corpuscle on the same side.

It is impossible to overrate the value of these discoveries; for if they are truths, the problem of nervous action is limited to these inquiries: *(a)* What are the properties of ganglionic corpuscles? *(b)* What are the properties of their two, or three, commissural processes? For we are already pretty well acquainted with the properties of the sensory and motor processes.

A short account was next given of the physical and physiological phenomena exhibited by active and inactive nerve; and the phenomena exhibited by active nerve were shown to be so peculiar as to justify the application of the title of "nerve force" to this form of material energy.

It was next pointed out that this force must be regarded as of the same order with other physical forces. The beautiful methods by which Helmholtz has determined the velocity (not more than about 80 feet in a second in the frog), with which the nervous force is propagated were explained. It was shown that nerve force is not electricity, but two important facts were cited to prove that the nerve force is a correlate of electricity, in the same sense as heat and magnetism are said to be correlates of that force. These facts were, firstly, the "negative deflection" of Du Bois Raymond, which demonstrates that the activity of nerve affects the electrical relations of its particles; and secondly, the remarkable experiments of Eckhard (some of which the speaker had exhibited in his Fullerian course) which prove that the transmission of a constant currrent along a portion of a motor nerve so alters the molecular state of that nerve as to render it incapable of exciting contraction when irritated.

These facts, even without those equally important though less thoroughly understood experiments of Ludwig and Bernard, which appear to indicate a direct relation between nerve force and chemical change, seem sufficient to prove that nerve force must henceforward take its place among the other physical forces.

This then is the present state of our knowledge of the structure and functions of nerve. We have reason to believe in the existence of a nervous force, which is as much the property of nerve as magnetism is of certain ores of iron; the velocity of that force is measured; its laws are, to a certain extent, elucidated; the structure of the apparatus through which it works promises soon to be

unravelled; the directions for future inquiry are limited and marked out; the solution of all problems connected with it is only a question of time.

Science may be congratulated on these results. Time was when the attempt to reduce vital phenomena to law and order, was regarded as little less than blasphemous: but the mechanician has proved that the living body obeys the mechanical laws of ordinary matter; the chemist has demonstrated that the component atoms of living beings are governed by affinities, of one nature with those which obtain in the rest of the universe; and now the physiologist, aided by the physicist, has attacked the problem of nervous action—the most especially vital of all vital phenomena—with what result has been seen. And thus from the region of disorderly mystery, which is the domain of ignorance, another vast province has been added to science, the realm of orderly mystery.

[T. H. H.]

WEEKLY EVENING MEETING,

Friday, May 22.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Edward Vivian, Esq. M.A. M.R.I.

On Meteorology, with Observations and Sketches taken during a Balloon Ascent.

A series of curves, showing the results of daily observations since 1842, and contributed during the last six years to the Meteorological Reports of the Registrar-General, were exhibited. From these the following summary of the climate of the south-eastern coast of Devon, as compared with the averages of England, was deduced—

	Mean Temp.	Max. Temp.	Min. Temp.	Daily range.	Days of rain.	Inches of rain.	Vapour in cubic foot of air.	Vapour required to produce saturation.	Mean humidity.
Torquay .	50°·3	76°	27°	9°·9	155	27·8	3·4	·9	·72
England .	48°·3	83°	15°	14°·5	170	25·5	3·4	·7	·82

The errors in medical and other works were referred to, especially in regard to the fall of rain, which is nearly double, both in amount and duration, on Dartmoor as compared with the south-eastern coast, from Exmouth to the Start Point, where the humidity of the air is also proportionally less, being, as stated above, the same in *absolute* amount with the average of England, and *sensibly* less in the proportion of 7 to 9. The climate of that coast was shown to be cool and dry in summer, but comparatively humid, as well as warm, in winter, owing to the influence of the sea, which retains a more uniform temperature, exhaling moisture in dry cold weather, but acting as a condenser whenever its temperature is below the dew-point of the air.

A set of instruments were exhibited, which gave, approximately, the following results from one monthly observation:—The maximum and minimum temperature; the maximum, minimum, and mean humidity; the greatest influence, and the duration of sunshine; the amount and duration of rain. The principle of most of these was founded upon the atmometer, with a combination of the wet and dry bulb and differential thermometers. By curves, exhibiting the fluctuations of the barometer, and the character of the weather, was shown how important it was to ascertain also the hygrometrical condition of the atmosphere, the barometer frequently rising before rains from the east. This diagram also proved how little influence the moon exerts, and the fallacy of the generally received opinion that its changes determine the subsequent character of the weather.

In conclusion, a narrative was given of a balloon ascent, illustrated by drawings of aerial phenomena, from sketches taken on the spot. The chief peculiarities of these were, the altitude of the horizon, which remained practically on a level with the eye at an elevation of two miles, causing the surface of the earth to appear concave instead of convex, and to recede during the rapid ascent, whilst the horizon and the balloon seemed to be stationary:—the definite outlines and pure colouring of objects directly beneath, although reduced to microscopic proportions, occasioned by the absence of refraction and dispersion of the coloured rays when passing perpendicularly through media of differing densities, which, at an angle, produce aerial perspective:—the rich combination of rays bursting through clouds, and having the sun's disc for their focus, contrasted with shadows upon the earth which radiate from a vanishing point on the horizon, the narrow shadows of clouds and eminences, such as Harrow and Richmond, being projected several miles, as seen in the lunar mountains: the magnificent Alpine scenery of the upper surfaces of cloud, still illumined, at high altitudes, by the cold silvery ray, contrasted with the rich hues of clouds at lower levels, and the darkness of the earth after sunset.

At higher altitudes than could be attained, and above the level of perpetual congelation, were the beautiful cirrus clouds, com-

posed of snow crystals, in every form and rich developement of the original hexagon, affording the materials for a new æra in architecture, and designs from Nature's hand for a crystal palace.

In acoustics, several interesting phenomena were noticed. The sound of London rolled westward as far as its smoke, but was lost above the clouds, where the most intense silence prevailed, as also near the surface of the earth, showing that sound ascends.

The electrical phenomena of lightning, hail, the peculiar forms of thunder clouds, and the aurora borealis, were beautifully illustrated with the instruments of the Institution; and photographs of natural clouds were exhibited, as also a method of introducing them by a second's negative in printing landscapes.

[E. V.]

WEEKLY EVENING MEETING,

Friday, May 29.

SIR HENRY HOLLAND, BART. M.D. F.R.S. Vice-President, in the Chair.

PROFESSOR A. J. SCOTT,
(OF OWEN'S COLLEGE, MANCHESTER.)

On Physics and Metaphysics.

[*No Abstract received.*]

GENERAL MONTHLY MEETING,

Monday, June 1.

WILLIAM POLE, ESQ. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Charles Tilston Bright, Esq. and
A. Colyar, Esq.

were duly *elected* Members of the Royal Institution.

Edmund Packe, Esq.

was *admitted* a Member of the Royal Institution.

The following PRESENTS were announced, and the thanks of the Members returned for the same :—

FROM—

Accademia dei Georgifili, Florence—Degli Studij e delle Vicende Sommario Storico. 8vo. Firenze, 1856.

Accademia Pontificia de' Nuovi Lincei, Roma—Atti, Anno VI. Sessione 1-5. 4to. 1855-6.

Astronomical Society, Royal—Monthly Notices, Vol. XVII. No. 7. 8vo. 1857.

Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for May 1857. 8vo.

Blashfield, J. M. Esq. M R.I.—Selection of Vases, Statues, &c. from Terracottas. 4to. 1857.

Boosey, Messrs. (the Publishers)—The Musical World for May 1857. 4to.

British Architects, Royal Institute of—Proceedings in May 1857. 4to.

Busk, Mrs. Wm. (the Author)—Mediæval Popes, Emperors, Kings, and Crusaders. 4 vols. 12mo. 1854.

De la Rue, Warren, Esq. Ph.D. M.R.I.—Saturn as seen through a Newtonian Equatorial, 13 in. aperture, Mar. 27, 1856.

Jupiter, as seen, Oct. 25, 1856.

Dilettanti, Society of—Historical Notices of the Society of Dilettanti. 4to. 1855.

Editors—The Medical Circular for May 1857. 8vo.

The Practical Mechanic's Journal for May 1857. 4to.

The Journal of Gas-Lighting for May 1857. 4to.

The Mechanic's Magazine for May 1857. 8vo.

The Athenæum for May, 1857. 4to.

The Engineer for May, 1857. fol.

The Artisan for May 1857.

Faraday, Professor, D.C.L. F.R.S. &c.—Königliche Preussischen Akademie —Berichte, 1856, Feb. 1857.

Forrester, the Baron, M.R.I. (the Author)—Memoria sobre o Curativo da Molestia nas Videiras [and other Papers]. 8vo. Porto, 1857.

Geographical Society, Royal—Journal. Vol. XXVI. 8vo. 1857.

Geological Survey of India—Memoirs. Vol. I. Part 1. 8vo. Calcutta, 1856.

Graham, George, Esq. (Registrar-General)—Reports of the Registrar-General for May 1857. 8vo.

Holland, Sir Henry, Bart. M.D. F.R.S. M.R.I.—Army Meteorological Registry from 1843 to 1854. 4to. Washington, U.S., 1855.

Annals of the Astronomical Observatory of Harvard College, U.S. Vol. I. Part 1. 4to. 1856.

Hope, A. J. Beresford, Esq. M.P. (the Author)—Public Offices and Metropolitan Improvements. 8vo. 1857.

Kaiserliche Geologische Reichsanstalt, Wien, (through M. W. Haidinger)—Abhandlungen, Band 1-3. 4to. 1852-6.

Jahrbuch, 1-7. 8vo. 1852-56.

Uebersicht der Resultate Mineralogischer Forschungen von Dr. G. A. Kenngott. 3 Bande. 1844-52. 4to. 1852-4.

Katalog der Bibliothek des K. K. Hof-Mineralien Cabinets in Wien, von P. Partsch. 4to. 1851.

Naturwissenschaftliche Abhandlungen, gesammelt und herausgegeben von W. Haidinger. Vol. 1-4. 4to. 1847-51.

Berichte herausgegeben von W. Haidinger. Bande 1-7. 8vo. 1847-51.

Kerr, Mrs. Alexander, M.R.I.—La Normandie Souterraine, par l'Abbé Cochet. 8vo. Paris, 1855.

Sepultures Gauloises, Romaines, Franques, et Normandes, par l'Abbé Cochet. 8vo. Paris, 1857.

Newton, Messrs.—London Journal (New Series), May 1857. 8vo.

Nicholson, Rev. Dr. H. J. (the Author)—The Abbey of St. Alban. 8vo. 1857.

Normandy, A. M.D. (*the Author*), *and Mr. George Knight* (*the Publisher*)—The Chemical Atlas or Tables: and a Dictionary of Reagents. fol. and 16to. 1857.
Novello, Mr. (*the Publisher*)—The Musical Times, for May 1857. 4to.
Petermann, A. Esq. (*the Author*)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1857. Heft 2. 4to. Gotha, 1857.
Photographic Society—Journal, No. 54. 8vo. 1856.
Reeves, Charles E. M.D. (*the Author*)—Diseases of the Stomach and Duodenum. 12mo. 1856.
Rico y Sinobas, Don Manuel (*the Author*)—Resumen de los Trabajos Meteorologicas. 4to. Madrid, 1857.
Royal Dublin Society—Journal, Nos. 4 and 5. 8vo. 1857.
Royal Society of London—Proceedings, No. 25. 8vo. 1857.
Society of Arts—Journal for May 1857. 8vo.
South, John F. Esq. (*the Author*)—Facts relating to Hospital Nurses. 8vo. 1857.
Vincent, B. Assist. Sec. R.I.—Portuguese Grammar, by A. Vieyra. 8vo. 1768.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen. Jan. und Feb. 1857. 4to.
Webster, John, M.D. F.R.S. M.R.I. (*the Author*)—Notes on Belgian Lunatic Asylums. 8vo. 1857.
Yates, James, Esq. F.R.S. M.R.I.—Report of the International Decimal Association. 8vo. 1857.

WEEKLY EVENING MEETING,

Friday, June 5.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

John Tyndall, Esq. F.R.S.
PROFESSOR OF NATURAL PHILOSOPHY, R.I.

On M. Lissajous' Acoustic Experiments.

The speaker briefly noticed the physical cause of musical sound; referring to the bell, the tuning fork, the tended string, &c., as sources of vibration. The propagation of impulses through the atmosphere to the tympanum was illustrated by causing a brass rod to vibrate longitudinally: a disk was fixed to the end of the rod perpendicular to its length, and this disk, being held several feet above a surface of stretched paper on which sand was strewn, communicated its motion through the air to the paper, and produced a complex nodal figure of great beauty. Optical means had been resorted to by Dr. Young, and more especially by Mr. Wheatstone, in the study of vibratory movements. M. Lissajous had extended and systematised the principle; and had exhibited

his experiments before the Société d'Encouragement, and more recently before the Emperor of the French. When he became acquainted with the speaker's intention to introduce these experiments at the Royal Institution, he in the most obliging manner offered to come to London and make them himself. This offer was accepted, and the speaker also congratulated the audience on the presence of M. Duboscq, who took charge of his own electric lamp; this being the source of light made use of on the occasion.

The experiments proceeded in the following order :—

1. A sheaf of light was thrown from the lamp upon a mirror held in the speaker's hand: on moving the mirror with sufficient speed the beam described a luminous ring upon the ceiling. The persistance of impressions upon the retina was thus illustrated.

2. A tuning fork had a pointed bit of copper foil attached to one of its prongs: the fork being caused to vibrate by a violin bow the metallic point moved to and fro, and being caused to press gently upon a surface of glass coated with lamp black, the fork being held still, a fine line of a length equal to the amplitude of the vibrations was described upon the glass; but when at the same time the whole fork was drawn backwards with sufficient speed, a sinuous line was described upon the glass. The experiment was made by placing the coated glass before the lamp; having a lens in front of it, and bringing the surface of the glass to a focus on a distant screen. On drawing the fork over the surface in the manner described, the figure started forth with great beauty and precision. By causing a number of forks to pass at the same time over the coated glass, the relations of their vibrations were determined by merely counting the sinuosities. The octave, for example, had double the number of its fundamental note.

3. This was the first of the series of M. Lissajous' experiments. A tuning fork, with a metallic mirror attached to one of its prongs, was placed in front of the lamp; an intense beam of light was thrown on the mirror, and reflected back by the latter. This reflected beam was received on a small looking-glass, held in the hand of the experimenter, from which it was reflected back upon the screen. A lens being placed between the lamp and tuning fork, a sharply defined image of the orifice from which the light issued was obtained. When a violin bow was drawn across the fork, this image elongated itself to a line. By turning the mirror in the hand, the image upon the screen was resolved into a bright sinuous track, many feet in length.

4. A tuning fork was placed before the lamp, as in the last experiment. But instead of receiving the beam reflected from the mirror of the fork upon a looking-glass, it was received upon the mirror of a second fork, and reflected by the latter upon the screen. When one fork was excited by a bow, a straight line described itself upon the screen, when the other fork was subsequently excited, the figure described was that due to the combination of the vibra-

tions of both the forks. This is the principle of the entire series of experiments now to be referred to.

When a single fork vibrates, the image which it casts upon the screen is elongated in a direction parallel to the prong of the fork. In order to have the vibrations rectangular, one fork stood upright, the other was fixed horizontally, in a vertical stand, in the following experiments.

5. Two forks, in perfect unison with each other, were placed in the positions described, and caused to vibrate simultaneously. If both forks passed their position of equilibrium at the same instant, that is, if there was no difference of phase, the figure described was a straight line. When the difference of phase amounted to one-fourth, the figure was a circle: between these it was an ellipse. The perfect unison of the two forks was proved by the immobility of the figure upon the screen. On loading one of them with a little weight, the figure no longer remained fixed but passed from the straight line through the ellipse to a circle, thence back through the ellipse to the straight line. So slight is the departure from unison which may be thus rendered visible, that M. Lissajous states that it would be possible to make evident to a deaf person a discrepancy of one vibration in thirty thousand.

6. Two forks, one of which gave the octave of the other, were next made use of. When there was no difference of phase, the figure described upon the screen resembled an 8. If the unison was perfect, the figure, as in the former case, was fixed; but when the unison was disturbed, the figure passed through the changes corresponding to all possible differences of phase. The loops of the 8 became distorted, formed by superposition a single parabola, opened out again, became again symmetrical, and so on.

7. The fifth of the octave, the major third, and other combinations succeeded, the figures becoming more and more complex as the departure from simple relations between the vibrations increased.

8. Finally, two forks which, when sounded together, gave audible beats, were placed both upright upon the table. The beam reflected from the mirror of one was received upon that of the other, and reflected upon the screen. When both forks were sounded, they sometimes conspired to elongate the image; sometimes they opposed each other, and thus a series of elongations and shortenings addressed the eye at exactly the same intervals in which the beats addressed the ear.

At the conclusion of this beautiful series of experiments, which, thanks to the skill of those who performed them, were all successful, on the motion of Mr. Faraday, the thanks of the meeting were unanimously voted to M.M. Lissajous and Duboscq, and communicated to those gentlemen by his Grace the President.

[J.T.]

WEEKLY EVENING MEETING,

Friday, June 12.

Sir Benjamin Collins Brodie, Bart. D.C.L. F.R.S. Vice-President, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On the Relations of Gold to Light.

This subject was brought forward on the 13th of June of last year, and in the account of that evening, at page 310, vol. ii. of the Proceedings of the Royal Institution, will be found a description of some of the proofs and effects then referred to and illustrated; the following additional remarks will complete the account up to this time. The general relations of *gold leaf* to light were described in the former report. Since then, pure gold leaf has been obtained through the kindness of Mr. Smirke, and the former observations verified. This was the more important in regard to the effect of heat in taking away the green colour of the transmitted light, and destroying to a large extent the power of reflexion. The temperature of boiling oil, if continued long enough, is sufficient for this effect; but a higher temperature (far short of fusion) produces it more rapidly. Whether it is the result of a mere breaking up by retraction of a corrugated film, or an allotropic change, is uncertain. Pressure restores the green colour; but it also has the like effect upon films obtained by other processes than beating. Corresponding results are produced with other metals.

As before stated, *films* of gold may be obtained on a weak solution of the metal, by bringing an atmosphere containing vapours of phosphorus into contact with it. They are produced also when small particles of phosphorus are placed floating on such a solution; and then, as a film differing in thickness is formed, the concentric rings due to Newton's thin plates are produced. These films transmit light of various colours. When heated they become amethystine or ruby; and then when pressed, become green, just as heated gold leaf. This effect of pressure is characteristic of metallic gold, whether it is in leaf, or film, or dust.

Gold wire, separated into very fine particles by the electric *deflagration*, produces a deposit on glass, which, being examined, either chemically or physically, proves to be pure metallic gold.

This deposit transmits various coloured rays: some parts are grey, others green or amethystine, or even a bright ruby. In order to remove any possibility of a compound of gold, as an oxide, being present, the deflagrations were made upon topaz, mica, and rock crystal, as well as glass, and also in atmospheres of carbonic acid and of hydrogen. Still the results were the same, and ruby gold appeared in one case as much as in another. Being heated, all parts of the deposit became of an amethystine or ruby colour; and by pressure these parts could be changed so as to transmit the green ray.

The production of *fluids*, consisting of very finely divided particles of gold diffused through water, was spoken of before. These fluids may be of various colours by transmitted light from ruby to blue; the effects being produced only by diffused particles of metallic gold. If a drop of solution of phosphorus in bisulphide of carbon be put into a bottle containing a quart or more of very weak solution of gold, and the whole be agitated, the change is brought about sooner than by the process formerly described; or if a solution of phosphorus in ether be employed, very quickly indeed; so that a few hours' standing completes the action. All the preparations have the same qualities as those before described. The differently coloured fluids may have the coloured particles partially removed by filtration; and so long as the particles are kept by the filter from aggregation, they preserve their ruby or other colour unchanged, even though salt be present. If fine isinglass be soaked in water, then warmed to melt it, and one of these rich fluids be added, with agitation, a ruby jelly fluid will be obtained, which, when sufficiently concentrated and cold, supplies a tremulous jelly; and this, when dried, yields a *hard ruby gelatine*, which being soaked in water, becomes tremulous again, and by heat and more water yields a ruby fluid. The dry hard ruby jelly is perfectly analogous to the well known ruby glass, though often finer in colour; and both owe the colour to particles of metallic gold. Animal membranes may in like manner have ruby particles diffused through them, and then are perfectly analogous in their action on light to the gold ruby glass, and from the same cause.

When a leaf of beaten gold is held obliquely across a ray of common light, it *polarizes* a portion of it; and the light transmitted is polarized in the same direction as that transmitted by a bundle of thin plates of glass; the effect is produced by the heated leaf as well as by the green leaf, and does not appear to be due to any condition brought on by the heating or to internal structure. When a polarized ray is employed, and the inclined leaf held across it, the ray is affected, and a part passes the analyzer, provided the gold film is inclined in a plane forming an angle of 45° with the plane of polarization. Like effects are produced by the films of gold produced from solution and phosphorus, and also by the deposited dust of gold due to the electric discharge. The same effects are

Royal Institution of Great Britain.

GENERAL MONTHLY MEETING,

Monday, November 2, 1857.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Captain James Drew,
The Rev. Allen Trevelyan Cooper, M.A. and
Charles William Lancaster, Esq.

were duly *elected* Members of the Royal Institution.

The following PRESENTS were announced, and the thanks of the Members returned for the same :—

FROM—

Her Majesty's Government—Report of H.M.S. Commissioners on Lunatic Asylums in Scotland. 8vo. 1857.
Judicial Statistics. Part 1. fol. 1857.
(*Through Sir R. I. Murchison*)—Memoirs of the Geological Survey :—Mining Records, Mineral Statistics of the United Kingdom, for 1853–56. By R. Hunt. 8vo. 1855–7.
Geology of the Country round Cheltenham. By Edward Hull. 8vo. 1857.
Tertiary Fluvio-Marine Formation of the Isle of Wight. By Edward Forbes. 8vo. 1856.
Descriptive Guide to the Museum of Geology. By R. Hunt. 12mo. 1857.
Iron Ores of Great Britain. Part I. 8vo. 1856.

Lords Commissioners of the Admiralty—Tables de la Lune. Par P. A. Hansen. 4to. 1857.

Actuaries, Institute of—Assurance Magazine. Nos. 28, 29. 8vo. 1857.

Agricultural Society, Royal—Journal, Vol. XVIII. Part 1. 8vo. 1857.

Amsterdam Koninklijke Akademie van Wetenschappen—Verslagen: Letterkunde, Deel II.; St. 2, 3, 4; Natuurkunde, Deel V.; St. 2, 3; Deel VI. 8vo. 1856–7.
Octaviæ Querela, Carmen, auctore J. Van Leeuwen. 8vo. 1857.

Andrew, W. P. Esq. (*the Author*)—Indus Flotilla. 8vo. 1857.

Antiquaries, Society of—Archæologia. Vol. XXXVI. Part 2; Vol. XXXVII. Part 1. 4to. 1856–7.
Proceedings, Nos. 43–46. 8vo. 1855–7.

Asiatic Society of Bengal—Journal, Nos. 159–161. 8vo. 1857.

Astronomical Society, Royal—Memoirs, Vol. XXV. 4to. 1857.
Monthly Notices, Vol. XVI. 8vo. 1857.

Author—Procès de l'Ex-Ministre Hellénique, Gen. Spiro Milios. 8vo. Athènes, 1856.

Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for July to Oct. 1857. 8vo.

Boosey, Messrs. (*the Publishers*)—The Musical World for July to Oct. 1857. 4to.

British Architects, Royal Institute of—Proceedings in July 1857. 4to.
British Association—Report of the Twenty-sixth Meeting, held at Cheltenham, 1856. 8vo. 1857.
Burel, M. E.—Rapport sur l'Exposition Universelle de 1855. 8vo. Rouen, 1856.
Canada, Government of—Catalogue of the Library of the Parliament of Canada. 8vo. 1857.
Chemical Society—Journal, No. 39. 8vo. 1857.
Daubeny, Charles, M.D. F.R.S. (*the Author*)—Lectures on Roman Husbandry. 8vo. 1857.
Denison, E. Beckett, Esq. M.A. Q.C. M.R.I. (*the Author.*)—Clocks and Locks. 16to. 1857.
Two Lectures on Gothic Architecture. By G. G. Scott and E. B. Denison. 12mo. 1857.
Department of State, Washington, U.S.—Track Survey of the Rivers Paraña, Uruguay, &c. By Capt. Thos. Page. 1855.
Dublin Geological Society—Journal, Vol. VII. No. 4. 8vo. 1857.
Dublin Society, Royal—Journal, No. 6. 8vo. 1857.
Editors—The Medical Circular for July to Oct. 1857. 8vo.
The Practical Mechanic's Journal for July to Oct. 1857. 4to.
The Journal of Gas-Lighting for July to Oct. 1857. 4to.
The Mechanic's Magazine for July to Oct. 1857. 8vo.
The Athenæum for July to Oct. 1857. 4to.
The Engineer for July to Oct. 1857. fol.
The Artisan for July to Oct. 1857.
Faraday, Professor, D.C.L. F.R.S. &c. (*the Author*)—On the Relations of Gold to Light. (Phil. Trans.) 4to. 1857.
Königliche Preussischen Akademie, Berichte, Mai—Aug. 1857.
Oversigt over det Kongelige Danske Videnskabernes Selskab, Fordhandlinger, 1856. 8vo. 1856.
Tageblatt der 32te Versammlung Deutscher Naturforscher in Wien in 1856. 4to. 1857.
Memorie della Reale Accademia delle Scienze, Napoli, dal 1852. Vol. I. Fasc. 1 & 2. 4to. 1856-7.
Rendiconto della Società Reale Borbonica. Anno V. 1856. Bimestre Gennaio e Febbraio. 4to.
Akademie der Wissenschaften, Wien: Math. Nat. Classe: Denkschriften, Band XII. 4to.; Sitzungsberichte. Band XX., XXI, XXII., und XXIII. Heft I., und Register zu Band XI.-XX. 8vo. 1856-7.
Almanach, 1857. 12to.
Accademia de Belgique: Bulletins de la Classe des Sciences, 1855-56. 8vo. Bruxelles, 4to. 1856-57.
Academie di Torino; Memorie, Serie Seconda. Tomo XVI. 4to. 1857.
Società Italiana in Modena; Memorie, Tomo XXV. Parte 2. 4to. 1855.
M. Quetelet: Observations des Phénomènes Périodiques. 4to. 1857: Sur le Climat de la Belgique. 4to. Bruxelles, 1857.
Meteorological Papers, published by Authority of the Board of Trade. No. I. 4to. 1857.
Franklin Institute of Pennsylvania—Journal, Vol. XXXIII. No. 6; Vol. XXXIV. No. 1. 8vo. 1857.
Gellibrand, W. Esq.—Vocabulary of the Aborigines of Tasmania. By Joseph Milligan. fol. 1857.
Geographical Society, Royal—Proceedings. Nos. 9, 10. 8vo. 1857.
Geological Society—Journal, No. 51. 8vo. 1857.
Glasgow Philosophical Society—Constitution of the Society; and the Catalogue of the Library. 8vo. 1850-7.
Graham, George, Esq. (*Registrar-General*)—Reports of the Registrar-General for July to Oct. 1857. 8vo.
Graubündens Naturforschende Gesellschaft—Jahresberichte. Neue Folge I. und II. 8vo. 1854-6.

Griffith, C. D. Esq. M.P. M.R.I. (the Author)—Speech on the Euphrates Railway and the Suez Canal. 8vo. 1857.
Hofmann, Dr. A. W. F.R.S. and H. M. Witt, F.C.S. (the Authors)—Metropolitan Drainage—Report of Chemical Investigations. fol. 1857.
Lewin, Malcolm, Esq. M.R.I. (the Editor)—Causes of the Hindu Revolt. By a Hindu of Bengal. 8vo. 1857.
Low, Mr. S.—Catalogue of American Books. 8vo. 1856.
Lubbock, John, Esq. M.R.I.—On reproduction in Daphnia, and on the Ephippium. (Phil. Trans.) 1857.
Murchison, Sir R. I. F.R.S. M.R.I. (the Author)—Notices of the Life of Dr. Buckland and the Earl of Ellesmere. 8vo. 1857.
Address to the Royal Geographical Society, May 25, 1857. 8vo.
Newton, Messrs.—London Journal (New Series), July to Oct. 1857. 8vo.
Novello, Mr. (the Publisher)—The Musical Times, for July to Oct. 1857. 4to.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1857. Heft 3–8. 4to. Gotha, 1857.
Photographic Society—Journal, Nos. 56–59. 8vo. 1857.
Rogers, J. W. C.E.—Facts and Fallacies of the Sewerage System of London, &c. 8vo 1857.
Royal Society of London—Philosophical Transactions, Vol. CXLVI. Parts 2 and 3; and Vol. CXLVII. Part 1. 4to. 1856–57.
Proceedings, Nos. 25–27. 8vo. 1857.
Society of Arts—Journal for July to Oct. 1857. 8vo.
Statistical Society—Journal, Vol. XX. Part 3. 8vo. 1857.
St. Petersbourg, Académie Impériale de—Mémoires: Sciences Naturelles, Tome VII., Sciences Politiques, Tome VIII. Divers Savans, Tome VII. 4to. 1854–5.
Compte Rendu, 1852–5. 8vo.
Bulletin Math.-Phys. Vol. XV. 4to. 1857.
Taylor, Rev. W., F.R.S. M.R.I.—Weber's Volkskalender für 1858. 12mo.
Vereins zur Beförderung des Gewerbfleisses in Preussen—März zu Aug. 1857. 4to.
Vincent, B. Assist. Sec. R.I. (the Editor)—Haydn's Dictionary of Dates. Eighth Edition. 8vo. 1857.
Warburton, Henry, Esq. F.R.S. M.R.I.—Report of the Senate of the University of London on the amended Draft Charter. 8vo. 1857.
Ward, F. O. Esq. (the Author)—Discours sur Bienfaisance Nationale, &c. 8vo. 1857.
Sur la Purification de l'Eau. 8vo. 1857,
Wrey, J. W. Esq. M.R.I.—Treatise on the Law of Tithes. By T. Cunningham. Fourth Edition. 8vo. 1777.
Blackstone's Commentaries on the Laws of England. Edited by E. Christian. 4 vols. 8vo. 1803.
Pleadings in Parliament in the Reigns of Edward I. and II. By W. Ryley. fol. 1661.
Collection of Cases of Privilege of Parliament from the Earliest Records to 1628. By J. Hatsell. 4to. 1766.
Sir R. Adair on the Negotiation for the Peace of the Dardanelles, 1808–9. 8vo. 1845.
Zoological Society of London—Transactions, Vol. IV. Part 4. 4to. 1857.
Proceedings, Nos. 314–333. 8vo. 1856–57.

GENERAL MONTHLY MEETING,

Monday, December 7.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Charles Brooke, Esq. F.R.S.

was duly *elected* a Member of the Royal Institution.

Neil Arnott, M.D. F.R.S.

was *admitted* a Member of the Royal Institution.

The Secretary reported that the following Arrangements had been made for the Lectures before Easter, 1858:—

Six Lectures on Static Electricity (adapted to a Juvenile Auditory), by Michael Faraday, Esq. D.C.L. F.R.S. &c. Fullerian Professor of Chemistry, R.I.

Twelve Lectures on the Principles of Biology, by Thomas Henry Huxley, Esq. F.R.S. Fullerian Professor of Physiology, R.I.

Ten Lectures on Heat, considered as a Mode of Motion, by John Tyndall, Esq. F.R.S. Professor of Natural Philosophy, R.I.

Ten Lectures on the Chemistry of the Elements which circulate in Nature, by Charles L. Bloxam, Esq. Professor of Practical Chemistry, King's College, London.

The following Presents were announced, and the thanks of the Members returned for the same:—

From—

Hon. East India Company—Bombay Magnetical and Meteorological Observations for 1854 and 1855. 4to.

American Academy of Sciences—Proceedings, Vol. III. Nos. 24-31. 8vo. 1856.

Memoirs, New Series, Vol. VI. Part 1. 4to. 1857.

American Philosophical Society—Proceedings, No. 56. 8vo. 1857.

Astronomical Society, Royal—Monthly Notices, Vol. XVII. No. 9. 8vo. 1857.

Athenæum Club—List of Members, &c. 16to. 1857.

Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for Nov. 1857. 8vo.

Boosey, Messrs. (the Publishers)—The Musical World for Nov. 1857. 4to.

Boston Society of Natural History—Proceedings, Vol. V. Nos. 21-26. Vol. IV. Nos. 1-10. 8vo. 1856-7.

British Architects, Royal Institute of—Proceedings in Nov. 1857. 4to.

De Broch, M., Ministre des Finances de Russie—Dr. C. H. Pander, Monographien über der Fossilen Fische des Silurischen Systems der Russisch-Baltischen Gouvernements; und über die Placodermen des Devonischen Systems. 4to. St. Petersburg, 1857.

Edinburgh Royal Observatory—Astronomical Observations. Vol. XI. 4to. 1857.

Edinburgh, Royal Society of—Transactions. Vol. XXI. Part 4. 4to. 1857. Proceedings, No. 47. 8vo. 1856–7.

Editors—The Medical Circular for Nov. 1857. 8vo.
The Practical Mechanic's Journal for Nov. 1857. 4to.
The Journal of Gas-Lighting for Nov. 1857. 4to.
The Mechanic's Magazine for Nov. 1857. 8vo.
The Athenæum for Nov. 1857. 4to.
The Engineer for Nov. 1857. fol.
The Artisan for Nov. 1857.
St. James's Medley, No. 12. 8vo. 1857.

Faraday, Professor, D.C.L. F.R.S. &c.—Abhandlungen der Akademie der Wissenschaften zu Berlin, 1856. 4to. 1857.
Akademie der Wissenschaften, Wien: Math. Nat. Classe, Denkschriften, Band XIII. 4to. Sitzungsberichte, Band XXIII. Heft 2: Band XXIV. Heft 1, 2. 8vo. 1857.

Franklin Institute of Pennsylvania—Journal, Vol. XXXIV. Nos. 2–4. 8vo. 1857.

Galbraith, Rev. J. A. and Rev. S. Haughton (the Authors)—Manuals of Arithmetic, Plane Trigonometry, Mechanics, Euclid (Books 1–3), Hydrostatics, Optics, and Astronomy. 16to. 1855–7.

Geological Society—Quarterly Journal, No. 52. 8vo. 1857.

Graham, George, Esq. (Registrar-General)—Reports of the Registrar-General for Nov. 1857. 8vo.
Eighteenth Annual Report. 8vo. 1857.

Huxley, Professor T. H. F.R.S.—On Glaciers: by Professors Tyndall and Huxley. (Phil. Trans.) 4to. 1857.

Jones, H. Bence, M.D. F.R.S. M.R.I. (the Editor)—The Chemistry of Wine. By G. J. Mulder. 16to. 1857.

Lankester, Edwin, M.D. F.R.S. (the Translator)—On the Animal and Vegetable Parasites of the Human Body. By F. Küchenmeister. Vol. I. 8vo. 1857.

Lewin, Malcolm, Esq. M.R.I. (the Author)—Torture in Madras. 2nd Edition. 8vo. 1857.

Linnean Society—Journal, No. 6. 8vo. 1857.

Lissajous, M. J. (the Author)—Mémoire sur l'Etude Optique des Mouvements Vibratoires. 8vo. Paris, 1857.

Liverpool Literary and Philosophical Society—Proceedings, No. 11. 8vo. 1857.

Madrid Real Academia de Ciencias—Memorias, Tomo IV. Parte 2. 4to 1857.

Manchester Literary and Philosophical Society—Memoirs, New Series. Vol. XIV. 8vo. 1857.
Meteorological Observations and Essays. By John Dalton. 2nd Edition. 8vo. 1834.
New System of Chemical Philosophy. By John Dalton. Part 1. 2nd Edition. Parts 2 and 3. 8vo. 1810–27.
Three Chemical Papers. By John Dalton. 8vo. 1840–2.

Newton, Messrs.—London Journal (New Series), Nov. 1857. 8vo.

Novello, Mr. (the Publisher)—The Musical Times, for Nov. 1857. 4to.

Photographic Society—Journal, No. 60. 8vo. 1857.

Portlock, Col. J. E. R.E. (the Author)—Address to the Geological Society, Feb. 20, 1857. 8vo.

Rennie, James, Esq. F.R.S. M.R.I.—Mathematical Treatises. By the Rev. J. West. 8vo. 1838.
Elements of Christian Theology. By Bishop Pretyman [Tomline]. 2 vols. 8vo. 1799.
Traité de Trigonométrie, par S. F. Lacroix. 8vo. Paris, 1837.
Précis de Géométrie, par A. J. Vincent. 8vo. Paris, 1837.
Traité de Géométrie Descriptive, par L. Lefébure de Fourcy. 8vo. Paris, 1837.
Royal College of Surgeons—Catalogue of the Library. 5 vols. 8vo. 1840-55.
Roma, Accademia Pontificia de' Nuovi Lincei—Atti, Anno VII. Sess. 1, 2. Anno. X. Sess. 1-5. 4to. 1856-7.
Sächsische Gesellschaft der Wissenschaften—Abhandlungen, Band VI. Hefte 1 und 2. 4to. 1857.
Berichte, 1856, Hefte 2. 1857, Hefte 1. 8vo.
Shaw, Alexander, Esq. M.R.I.— Report on Dr. Fell's Treatment of Cancerous Diseases at Middlesex Hospital. 8vo. 1857.
Smithsonian Institution, Washington—Tenth and Eleventh Annual Reports. 8vo. 1856-7.
Smithsonian Contributions, Vol. IX. 4to. 1857.
Researches on the Ammonio-Cobalt Bases; by W. Gibbs and S. Genth. 4to. 1856.
Society of Arts—Journal for Nov. 1857. 8vo.
Sopwith, Thos. Esq. M.R.I. (the Author)—Notes of a Visit to Egypt. 16to. 1857.
Streatfeild, J. F. Esq. (the Editor)—Ophthalmic Hospital Reports. No. 1. 8vo. 1857.
United States Coast Survey—Report for 1855.
Zoological Society—Proceedings, Nos. 334-338. 8vo. 1857.

1858.

WEEKLY EVENING MEETING,

Friday, January 22.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

PROFESSOR J. TYNDALL, F.R.S.

On some Physical Properties of Ice.

THE discourse was prefaced by some remarks on force in general; and more especial attention was afterwards directed to the force, or application of force, manifested in the phenomena of crystallization. Experimental illustrations were exhibited, and the speaker passed on to the particular case of crystallized water, or ice. Being

desirous of examining how the interior of a mass of ice was affected by a beam of radiant heat sent through it, he availed himself of the sunny weather of last September and October. The sunbeams being condensed by a lens, the concentrated beams were sent through slabs of ice, the arrangement being usually so made as to cause the focus to fall within the substance. The path of such a beam through the ice was observed to be instantly studded with lustrous spots, which increased in magnitude and number as the action continued. On examining the spots more closely, they were found to be flattened spheroids, and around each the ice was so liquified as to form a beautiful flower-shaped figure, possessing six petals. From this number there was no deviation. At first the edges of the liquid leaves were clearly defined; but a continuance of the action usually caused the edges to become serrated like those of ferns. When the ice was caused to move across the beam, or the beam caused to traverse different portions of the ice in succession, the sudden generation and crowding together of these liquid flowers, with their central spots shining with more than metallic brilliancy, was exceedingly beautiful.

A slab of ice was prepared, and placed in front of an electric lamp: by a lens placed in front of the slab, the latter was projected upon a screen; on sending a beam from the lamp through the ice, the formation of the flowers was rendered visible to the audience.

In almost all cases these flowers were formed in planes parallel to the surface of freezing; it mattered not whether the beam traversed the ice parallel to this surface, or perpendicular to it. Some apparent exceptions to this rule were found, which will form the subject of future investigation.

The general appearance of the shining spots at the centres of the flowers was that of the bubbles of air entrapped in the ice: to examine whether they contained air or not, portions of ice containing them were immersed in warm water. The ice surrounding the cavities melted, the latter instantly collapsed, and no trace of air rose to the surface of the water. A vacuum, therefore, had been formed at the centre of each spot; due, doubtless, to the well-known fact that the volume of water in each flower was less than that of the ice, by the melting of which the flower was produced.

The associated air-and-water cells, found in such numbers in the ice of glaciers, and observed by the speaker in lake ice, were next examined. Two hypotheses have been started to account for these cells. One attributes them to the absorption of the sun's heat by the air bubbles, and the consequent melting of the ice which surrounds them. The other hypothesis, which is a very reasonable one, supposes that the liquid in the cells never has been frozen, but has continued in the liquid condition from the névé or origin of the glacier downwards. Now if the water in the cells be due to the melting of the ice, the associated air must be in a rarefied condi-

tion, because the volume of the liquid is less than that of the ice which produced it; whereas, if the air be simply that entrapped in the snow of the névé, it will not be thus rarefied. Here, then, we have a test as to whether the water-cells have been produced by the melting of the ice.

Portions of ice containing these compound cells were immersed in hot water, the ice around the cavities being thus gradually melted away. When a liquid connexion was established between the bubble and the atmosphere, the former collapsed to a smaller bubble. In many cases the residual bubble did not reach the hundredth part of the magnitude of the primitive one. There was no exception to this rule, and it proves that the water of the cavities is really due to the melting of the adjacent ice.

The first hypothesis above referred to is that of M. Agassiz; which has been reproduced and subscribed to by the Messrs. Schlagintweit, and accepted generally as the true one. Let us pursue it to its consequences.

Comparing equal *weights* of air and water, experiment proves that to raise a given weight of water one degree in temperature, as much heat would be needed as would raise the same weight of air four degrees.

Comparing equal *volumes* of air and water, the water is known to be 770 times heavier than the air; consequently, for a given *volume* of air to raise an equal *volume* of water one degree in temperature, it must part with $770 \times 4 = 3080$ degrees.

Now the quantity of heat necessary to melt a given weight of ice would raise the same weight of water 142·6 Fahr. degrees in temperature. Hence to produce, by the melting of ice, an amount of water equal to itself in bulk, a bubble of air must yield up $3080 \times 142{\cdot}6$, or upwards of four hundred thousand degrees Fahrenheit.

This is the amount of heat which, according to the hypothesis of M. Agassiz and the Messrs. Schlagintweit, is absorbed by the bubble of the air in a short time under the eyes of the observer. That is to say, the air is capable of absorbing an amount of heat which, had it not been communicated to the surrounding ice, would raise the bubble to a temperature 160 times that of fused cast iron. Did air possess this enormous power of absorption it would not be without inconvenience for the animal and vegetable life of our planet.

The fact is, that a bubble of air at the earth's surface is unable, in the sightest appreciable degree, to absorb the sun's rays; for those rays before they reach the earth have been perfectly sifted by their passage through the atmosphere. The following experiment illustrative of this point, has been made by the speaker: the rays from an electric lamp were condensed by a lens, and the concentrated beam sent through the bulb of a differential thermometer. The heat of the beam was intense; still not the slightest effect was

produced upon the thermometer. In fact, all the rays that glass could absorb had been *absorbed by the lens*, and the heat consequently passed through the thin glass envelope of the thermometer, and the air within it, without imparting the slightest sensible heat to either.

The bubbles observed by the speaker, and those which occur in the deeper portions of glacier ice, he supposes to have been produced by heat which has been *conducted* through the substance without melting it. Regarding heat as a mode of motion, he shows that the liberty of liquidity is attained by the molecules at the surface of a mass of ice, before the molecules at the centre of the mass can attain this liquidity. Within the mass each molecule is controlled in its motion by the surrounding molecules. But if a cavity exist at the interior, the molecules surrounding that cavity are in a condition similar to those at the surface; and they are liberated by an amount of motion which has been transmitted through the ice without prejudice to its solidity. The conception is helped when we call to mind the transmission of motion through a series of elastic balls, by which the last ball of the series is detached, while the others do not suffer visible separation. The speaker, moreover, proves, by actual experiment, that the interior portion of a mass of ice may be liquified by an amount of heat which has been conducted through the exterior portions without melting them.

Now precisely the converse of this takes place when two pieces of ice, at 32° Fahr., with moist surfaces, are brought into contact. Superficial portions are by this act transferred to the centre, where a temperature of 32° is not sufficient to produce liquefaction. The motion of liquidity which the surfaces possessed before contact is now checked, and the pieces of ice freeze together. This appears to furnish a complete explanation of all the cases of this nature which have hitherto been observed.

The particles of a crushed mass of ice at 32°, or a ball of moist snow, may, it is now well known, be squeezed into slabs or cups of ice. That moisture is necessary here, and that the same agent is necessary in the conversion of snow into glacier ice, was proved by the following experiment. A ball of ice was cooled in a bath of solid carbonic acid and ether, and thus rendered perfectly dry. Placed in a suitable mould, and subjected to hydraulic pressure, the ball was crushed; but the crushed fragments remained as *white and opaque* as those of crushed glass. The particles, while thus dry, could not be squeezed so as to form pellucid ice, which is so easily obtained when the compressed mass is at a temperature of 32° Fahr.

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 29.

Sir Benjamin Collins Brodie, Bart., D.C.L. F.R.S.
Vice-President, in the Chair.

William Robert Grove, Esq. Q.C. V.P.R.S.

On Molecular Impressions by Light and Electricity.

The term *molecule* is used in different senses by different authors: by some it is employed with the same meaning as the word *atom*, *i.e.*, to signify an ultimate indivisible particle of matter; by others to signify a definite congeries of atoms forming an integral element of matter, somewhat as a brick may be said to be a congeries of particles of sand, but a structural element of a house.

The term is used this evening to signify the particles of bodies smaller than those having a sensible magnitude, or only as a term of contradistinction from masses. If there be any distinctive characteristic of the science of the present century as contrasted with that of former times, it is the progress made in molecular physics, or the successive discoveries which have shown that when ordinary ponderable matter is subjected to the action of what were formerly called the imponderables, the matter is molecularly changed. The remarkable relations existing between the physical structure of matter, and its effect upon heat, light, electricity, magnetism, &c., seems, until the present century, to have attracted little attention: thus, to take the two agents selected for this evening's discourse, Light and Electricity, how manifestly their effects depend upon the molecular structure of the bodies subjected to their influence? Carbon in the form of diamond transmits light but stops electricity. Carbon in the form of coke or graphite, into which the diamond may be transformed by heat, transmits electricity but stops light. All solid bodies which transmit light freely, or are transparent, are non-conductors of electricity, or may be said to be opaque to it; all the best conductors of electricity, as black carbon and the metals, are opaque or non-conductors of light.* Bodies which have a peculiar but definite and symmetrical structure, such as crystals, affect light definitely and in strict relation to their structure: witness the effects of polarized light on crystals; and there are not wanting instances

* It should be borne in mind that these terms are not absolute, but only express a high degree of approximation.

of similar relations between the structure of bodies and their transmission of electricity.

The converse of this class of effects, however, forms more properly the subject of this evening's communication, viz., the changes in the molecular structure of matter produced by Light and Electricity. The effect of light on plants, on their growth and colour, the bleaching effects of light on coloured bodies, the phosphorescence of certain substances by insolation or exposure to the sun, have long been known, and yet do not seem to have awakened in the minds of the ancient natural philosophers any notion of the general molecular effects of light. Leonard Euler alone conceived that light may be regarded as a movement or undulation of ordinary matter; and Dr. Young, in answer, stated as a most formidable objection, that if this view were correct all bodies should possess the properties of solar phosphorus, or should be thrown into a state of molecular vibration by the impact of light, just as a resonant body is thrown into vibration by the impact of sound, and thus give back to the sentient organ an effect similar to that of the original impulse.

In the last edition of his Essay on the "Correlation of Physical Forces," (1855, p. 131,) Mr. Grove has made the following remarks on this question: "To the main objection of Dr. Young that all bodies would have the properties of solar phosphorus if light consisted in the undulations of ordinary matter, it may be answered that so many bodies have this property, and with so great variety in its duration, that *non constat* all may not have it, though for a time so short that the eye cannot detect its duration; the fact of the phosphorescence by insolation of a large number of bodies is in itself evidence of the matter of which they are composed being thrown into a state of undulation, or at all events molecularly affected by the impact of light, and is therefore an argument in support of the view to which objection is taken." The above conjecture has been substantially verified by the recent experiments of M. Niepce de St. Victor, of which the following is a short *resumé*:—

An engraving which has been for some time in the dark is exposed to sunlight as to one half, the other half being covered by an opaque screen: it is then taken into a dark room, the screen removed, and the whole surface placed in close proximity to a sheet of highly sensitive photographic paper. The portion upon which the light has impinged is reproduced on the photographic paper, while no effect is produced by the portion which had been screened from light. White bodies produce the greatest effect, black little or none, and colours intermediate effects.

An engraving exposed as before, then placed in the dark upon white paper, conveys the impression to the latter, which will in its turn impress photographic paper.

Paper, in a tin case, exposed to sunlight, then covered up by a tin cover will, when opened in the dark, radiate from the aperture

phosphorescent force, and produce a circular mark on the photographic paper, and even impress on the latter the lines of an engraving interposed between it and the photographic surface.

Phosphorescent bodies produce similar effects in a greater degree, and bodies which intercept the phosphorescent effect intercept the invisible radiations. A design drawn by a fluorescent substance, such as a solution of sulphate of quinine on paper, is reproduced, the design being more strongly impressed than the residual parts of the paper.

Mr. Grove had little doubt that had the discourse been given in the summer instead of mid-winter, he could have literally realised in this theatre the Lagado problem of extracting sunbeams from cucumbers!

While fishing in the autumn, in the grounds of M. Seguin, at Fontenay, Mr. Grove observed some white patches on the skin of a trout, which he was satisfied had not been there when the fish was taken out of the water. The fish having been rolling about in some leaves at the foot of a tree, gave him the notion that the effect might be photographic, arising from the sunlight having darkened the uncovered, but not the covered portions of the skin. With a fresh fish a serrated leaf was placed on each side, and the fish laid down so that the one side should be exposed, the other sheltered from light: after an hour or so the fish was examined, and a well defined image of the leaf was apparent on the upper or exposed side, but none on the under or sheltered side. There was no opportunity of further experiment; but there seems little doubt of the effect being photographic, or an oxidation or deoxidation of the tissue determined by light.

Many important considerations might be suggested as deducible from the above results, as to the influence of light on health, both that of vegetables and animals. The effect of light on the healthy growth of plants is well known; and it is generally believed that dark rooms, though well heated and ventilated, are more "close" or less healthy than those exposed to light. When we consider the invisible phosphorescence which must radiate from the walls and furniture, when we consider the effects of light on animal tissue, and the probable ozonizing or other minute chemical changes in the atmosphere effected by light, it becomes probable that it is far more immediately influential on the health of the animate world than is generally believed.

The number of substances proved to be molecularly affected by light is so rapidly increasing, that it is by no means unreasonable to suppose that all bodies are in a greater or less degree changed by its impact.

Passing now to the effects of Electricity, every day brings us fresh evidence of the molecular changes effected by this agent. The electric discharge alters the constitution of many gases across which it is passed; and it was shown, that by passing it through

an attenuated atmosphere of the vapours of phosphorus, this element is changed by the electric discharge into its allotropic variety, which is deposited in notable quantity on the sides of the receiver. In this experiment, the transverse bands or striæ discovered by Mr. Grove, in 1852, are very strikingly shown. Not only is the gaseous intermedium thus affected, but the terminals from which the discharge appears to issue, are disintegrated, and their molecules projected. Some tubes, through the interior of which Mr. Gassiot had passed the discharge from Ruhmkorff's coil for a considerable time, were shown to be coated in the interior, for a notable space around the negative terminal, with a deposit of platinum, forming a reflecting surface like the back of a looking-glass. The vacuum in these tubes was Torricellian, the tubes having been hermetically sealed after the descent of the mercury, so as to cut them off from the mercurial surface. In these cases the electric discharge passes from metal to metal; but the glow which is seen on excited electrics, such as glass, was also shown by Mr. Grove to be accompanied with molecular change. Letters cut in paper, and placed between two well cleaned sheets of glass, formed into a Leyden apparatus by sheets of tin foil on their outer surfaces, and then electrified by connexion for a few seconds with a Ruhmkorff coil, had invisible images of the letters impressed upon the interior surfaces, which were rendered visible by breathing on them; and rendered visible, and at the same time permanently etched, by exposure, after electrization, to the vapour of hydrofluoric acid.

So, again, if iodized collodion be poured over the surface of glass having the invisible image, and then treated as for a photograph, and exposed to uniform daylight, the invisible image is ultimately developed in the collodion film: the invisible molecular change having been conveyed to the collodion, and rendering it, when nitrated, more sensitive to light in the parts where it has been in proximity to the electrical impression, than in the residual parts. Here we have a molecular change, produced first by electricity on the glass, then communicated by the glass to the collodion, then changed in character by light, and all this time invisible; and then rendered visible by pyrogallic acid, the developing chemical agent. Test papers between the plates of glass so electrized, show an acid, and also a bleaching re-action, probably due to the formation of nitrous acid and of ozone; and thus evidencing a chemical change in the elastic intermedium, as well as in the bounding surfaces: but the interior molecules of the glass appear also to partake of the effect, as the impressions are reproduced in many cases on the opposite surface of the glass.

Mr. Babbage had observed that some plates of glass which had formed the ornamented margin of an old looking-glass, and were backed by a design in gold leaf covered with plaster of Paris, showed, when this backing was removed by soft soap, an impression of the gold-leaf device, which was rendered visible by the breath on

the glass. Some of the plates had been kindly lent by him for this evening; and in one, Mr. Grove had removed a portion of the backing, and the continuation of the gilded design came beautifully out by breathing on the glass while in the frame of the electric lamp, and was projected (as were the previous electrical images) on a white screen. The effect on Mr. Babbage's plates may be also electrical, arising from the gold—a good conductor—acting as platinum does in the voltaic battery, and setting up a chemical action between the substance used for making the gold adhere and the glass, or between the constituents of the glass itself; but it would be hazardous, without further experiment, to express any confident opinion on this point.

Of the practical results to science of the molecular changes forming the subject of this evening's discourse, a beautiful illustration was afforded by the photographs of the moon by Mr. Warren De la Rue, which gave, by the aid of the electric lamp, images of the moon of six feet diameter, in which the details of the moon's surface were well defined,—the cone in Tycho, the double cone in Copernicus, and even the ridge of Aristarchus, could be detected. The bright lines, radiating from the mountains, were clear and distinct. A photograph of the planet Jupiter was also shown, in which the belts were very well marked, and the satellites visible. The following question was suggested by Mr. Grove. As telescopic power is known to be limited by the area of the speculum or object glass, even assuming perfect definition, as the light decreases inversely as the square of the magnifying power, a limit must be reached at which the minute details of an object become lost for want of light. Now, assuming a high degree of perfection in astronomical photographs, these may be illuminated to an indefinite degree of brilliancy by adventitious light. With a given telescope, could a better effect be obtained by illuminating the photographic image, and applying microscopic power to that, than by magnifying the luminous image in the usual way by the eye-glass of the telescope? Can the addition of extraneous light to the photograph permit a higher magnifying power to be used with effect than that which can be used to look at the image which makes the photographic impression? In other words, is the photographic eye more sensitive than the living eye; or can a photographic recipient be found which will register impressions which the living eye does not detect, but which, by increased light or by developing agents, may be rendered visible to the living eye? Much may be said, *pro* and *con*, on this question, and it probably can only be satisfactorily answered by experiment, when photographic science is sufficiently advanced.

The phenomena treated of this evening, which are a mere selection from a crowd of analogous effects, show that light and electricity, in numerous cases, produce a molecular change in ponderable matter affected by them. The modifications of the supposed imponderables themselves have long been the subjects of investigation;

the recent progress of science teaches us to look for the reciprocal effects on the matter affected by them.

Gases which have transmitted *light* are altered; as, for example, chlorine is rendered capable of combining directly with hydrogen; liquids are altered, peroxalate of iron is chemically changed, and gives off carbonic acid; and the light which has produced these effects is less able to produce them a second time. Solids are altered, as shown in the extensive range of photographic effects. So with *electricity*,—compound gases are changed chemically, as ammonia or atmospheric air; elementary gases are changed allotropically, as phosphorus vapour, or oxygen; liquids are changed, as in the decomposition of water and other electrolytes; and solids are changed, as in the projection of the particles of the terminals, and the impressions on the surfaces of electrics, shown this evening. Frictional electricity may itself be due to the rupture of cohesion between dissimilar molecules; at all events few, if any, electrical effects have not been proved to be accompanied with molecular changes; and we are daily receiving additions to those produced by light. So, again, iron, and other bodies, have their molecular structure changed by magnetism. Chemical affinity is universally, and heat generally, admitted to be an affection of ordinary matter. Mr. Grove feels deeply convinced that a dynamic theory, one which regards the imponderables as forces acting upon ordinary matter in different states of density, or as modes of motion, and not as fluids or entities, is the truest conception which the mind can form of these agents; but to those who are not willing to go so far, the ever increasing number of instances of such molecular changes affords a boundless field of promise for future investigation, for new physical discoveries and new practical applications.

The permanency of such changes also gives valuable means of reading, in the present state of matter, its past history; final or absolute knowledge on such subjects we cannot hope to obtain, but relative or approximate knowledge is as unlimited as is the degree of improvement in the powers attainable for its acquisition.

Note.—Since the above was written, the author has observed a case of molecular action which, in some respects, goes further than any yet recorded. He happened to procure a small Galilean telescope, or perspective glass, by Dollond, $6\frac{1}{2}$ inches focus, and $1\frac{3}{10}$ths aperture, of which the tripod stand was so arranged as to fold up and pack into the tube. When so packed it terminated opposite the object glass in a disc of brass, in the centre of which was an aperture $\frac{4}{10}$ths of an inch diameter, and in the centre of this the end of an iron screw, of $\frac{2}{10}$ths of an inch. The distance of the perforated disc from the inner surface of the object glass was $\frac{1}{8}$th of an inch. The impression of this disc and of the central pivot was delicately etched on the glass; the polished surface being disintegrated opposite the brass and iron, an *annulus*, opposite the space between

these, retaining its polish. The molecular change is extremely delicate, and can only be seen in certain inclinations to the light; it does not seem to affect the performance of the glass. Dollond died in 1761; but whether made by him or his son, the instrument bears internal evidence of being very old; and was represented as having been 40 years in the shop where it was bought. We have therefore an experiment of very long duration, and which presents these remarkable points: 1st, There is a notable distance between the radiating surfaces. 2ndly, The impression is permanently etched, and not capable of being removed by any cleaning of the surface. It would be out of place in this note to enter on the theory of this effect.

[W. R. G.]

GENERAL MONTHLY MEETING,

Monday, February 1.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Richard Corbet, Esq.
Roger Fenton, Esq.
Mervyn Hamilton, Esq.
William Augustus Hillman, Esq. F.R.C.S. and
John Leighton, jun. Esq. F.S.A.

were duly *elected* Members of the Royal Institution.

Charles Brooke, Esq. F.R.S.

was *admitted* a Member of the Royal Institution.

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM—
Actuaries, Institute of—Assurance Magazine. No. 30. 8vo. 1857.
Anonymous—Neufchâtel, and its Events since 1814. 8vo. 1857.
Asiatic Society of Bengal—Journal, No. 263. 8vo. 1857.
Astronomical Society, Royal—Monthly Notice, Dec. 1857.
Basel Naturforschende Gesellschaft—Verhandlungen, Viertes Heft. 8vo. 1857.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for Jan. 1858. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for Jan. 1858. 4to.
Bombay Geographical Society—Transactions, Vol. XIII. 8vo. 1857.

British Architects, Royal Institute of—Proceedings in Nov. and Dec. 1857, and Jan. 1858. 4to.
Cambridge Observatory—Astronomical Observations for 1849–51. Vol. XVIII. 4to. 1857.
Carrington, R. C. Esq. (the Author)—Catalogue of 3735 Circumpolar Stars observed at Redhill in 1854–56. fol. 1857.
Chemical Society—Journal, No. 40. 8vo. 1858.
De Brock, M., Ministre des Finances de Russie—Annales de l'Observatoire Physique Central de Russie. Par A. T. Kupffer. 1854. 2 vols. 4to. 1856.
Editors—The Medical Circular for Nov. and Dec. 1857, and Jan. 1858. 8vo.
The Practical Mechanic's Journal for Nov. and Dec. 1857, and Jan. 1858. 4to.
The Journal of Gas-Lighting for Nov. and Dec. 1857, and Jan. 1858. 4to.
The Mechanic's Magazine for Nov. and Dec. 1857, and Jan. 1858. 8vo.
The Athenæum for Nov. and Dec. 1857, and Jan. 1858. 4to.
The Engineer for Nov. and Dec. 1857, and Jan. 1858. fol.
The Artizan for Nov. and Dec. 1857, and Jan. 1858.
The Illustrated Inventor for Nov. and Dec. 1857, and Jan. 1858.
The Atlantis, No. 1. 8vo. 1858.
Faraday, Professor, D.C.L. F.R.S.—Bulletin de la Sociéte Vaudoise, Nos. 38—40. 8vo. Lausanne, 1856–57.
Königliche Preussischen Akademie,—Berichte, Sept. Oct. und Nov. 1857. 8vo.
Rapport sur l'Exposition Universelle, 1855, présenté à l'Empereur par S. A. I. le Prince Napoléon. 4to. Paris, 1857.
Franklin Institute of Pennsylvania—Journal, Vol. XXXIV. Nos. 5, 6. 8vo. 1857.
Geological Society—Proceedings, No. 1–8. 8vo. 1857.
Geological Society of Dublin—Journal, Vol. VII. Part 5. 8vo. 1857.
Geological and Polytechnic Society of the West Riding of Yorkshire—Report of Proceedings, 1856–7. 8vo.
Geologische Anstalt, Wien—Jahrbuch, 1857. No. 1. 4to. 1857.
Graham, George, Esq. (Registrar-General)—Reports of the Registrar-General for Nov. and Dec. 1857, and Jan. 1858. 8vo.
Hanwell Asylum, Committee of Visitors—Eleventh and Twelfth Reports. 12mo. 1856–57.
Johnson, Edmund C. Esq. M.R.I.—St. James's Medley, Nos. 9, 10, 12. 8vo. 1857.
Biographical Sketches of Great Monarchs, by Viscount Cranborne. 16to. 1853.
The Land of Silence and the Land of Darkness. By the Rev. B. G. Johns. 16to. 1857.
Linnean Society—Journal, No. 6. 8vo. 1857.
Transactions, Vol. XX. Part 2. 4to. 1857.
Leeds Philosophical and Literary Society—Annual Report, 1856–7. 8vo.
Lubbock, John, Esq. M.R.I. (the Author)—On Eight New Species of the Entomostraca. 8vo. 1857.
Macrory, Edmund, Esq. M.R.I. (the Author)—Reports of Patent Cases. Part 2. 8vo. 1857.
Newton, Messrs.—London Journal (New Series), Nov. and Dec. 1857, and Jan. 1858. 8vo.
Novello, Mr. (the Publisher)—The Musical Times, for Nov. and Dec. 1857, and Jan. 1858.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie, 1857. Heft 9, 10, 11. 4to. Gotha, 1857.
Photographic Society—Journal, Nos. 60, 61, 62. 8vo. 1857.
Royal Medical and Chirurgical Society—Transactions, Vol. XL. 8vo. 1857.
Catalogue of the Library. 8vo. 1856.
Royal Society—Proceedings, No. 28. 8vo. 1857.
St. Petersbourg, Academie Impériale de—Mémoires des Sciences Mathématiques. Tome VI. 4to. 1857.

Society of Arts—Journal for Nov. and Dec. 1857, and Jan. 1858. 8vo.
Sopwith, Thomas, Esq. F.R.S. M.R.I. (*the Author*)—Reminiscences of First Visits to Scotland, London, and the South-West of England in 1828, 1830, and 1833. 16to. 1847.
Treatise on Isometrical Drawing. 2nd Edition. 8vo. 1838.
Taylor, Rev. W. F.R.S.—Portraits. 16to. Leipzig, 1779.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Sept. and Oct. 1857. 4to.
Von Kersten, Charles, Esq. (*the Author*)—Reading made Easy (for German, English, and French, by New Characters.) 8vo. Brussels, 1857.
Wilson, Thomas, Esq. M.R.I.—Plans of the Lake of Haarlem and its Drying-up. 1843-55.
Yearsley, James, Esq. M.R.I. (*the Author*)—Controversy on the Artificial Tympanum. 8vo. 1858.
Zoological Society of London—Proceedings, Nos. 334-338. 8vo. 1857.

WEEKLY EVENING MEETING,

Friday, February 5.

The Lord Wensleydale, Vice-President, in the Chair.

Edwin Lankester, M.D. F.R.S. M.R.I.

On the Drinking Waters of the Metropolis.

In bringing the subject of the drinking waters of the metropolis before his audience, the lecturer stated that he wished to address them not as a chemist or a naturalist, but as a medical officer of health. He wished to make his lecture practical, and to answer the question, What water shall we drink? Water might be discoursed on as a solid, a liquid, or a vapour, and from every point of view it had deep interest for man. It was as one of the great factors of the organic kingdoms, that he must now regard it. Water was necessary to the formation of the tissues of both plants and animals. Some water plants consisted of from 90 to 95 per cent. of water, whilst Professor Owen had estimated the solid matter of a jelly-fish weighing two pounds, at sixteen grains. Seventy-eight parts in the hundred of blood, and seventy-two parts in the hundred of muscle was water. Most kinds of solid human food contained more than fifty per cent. of water.

Water was not only necessary to the formation of animal and vegetable tissues, but to the introduction into the system of *saline* and *organic* matters. The water of the blood held 420 grains of saline matters in solution, and the tissues also contained salts, such as phosphate of lime, which were introduced by the agency of water. The organised matters of the food of animals were taken

into the blood by the agency of water. Fats were reduced to soluble soaps, and proteinaceous matters were formed into soluble albumen before being taken into the system.

The great source of water for these purposes was the ocean, which spread over the surface of the earth, raised into the atmosphere by heat, and precipitated again by cold, formed snow and rain. These collecting on the surface of the earth, formed *rivers*, or penetrating the rocks, appeared again as *springs*. Both kinds of water contained *organic* and *inorganic* substances. The latter were dissolved, the former for the most part suspended. In river waters the inorganic matters varied according to the rocks over which they flowed. The Dee contained five grains in the gallon; the Exe, fifteen grains; the Wandle, seventeen; and the Thames, twenty. The organic matters of rivers are derived from the plants and animals which live in them, and from the vegetable and animals refuse cast into them.

Spring waters contained more and less inorganic matters than rivers. When the quantity was large or peculiar, they were called *mineral* waters. When the salts of lime abounded they were called *hard* waters; and when free from the salts of lime, and very large quantities of other salts, *soft* water. Spring waters generally contained less organic matters than river waters.

The water used in London for drinking purposes was obtained from both rivers and springs. The Thames and the New River, and partially other rivers, supplied the river water. The spring water was of two kinds. First, from surface wells, obtained by digging through the gravel which covered the London clay in the western parts of the metropolis, and into the clay itself. Secondly, from deep wells, which generally passed through the London clay and penetrated the chalk below. The surface wells received the soakage of the water which fell over London, and the water was contaminated by the contents of cesspools, drains, and sewers. The deep wells received their supply of water from the chalk which formed the sides of the great "London Basin." All these waters contain more or less of the following mineral constituents:—

1. *Carbonate of Lime*, of which 3 to 17 grains are contained in the gallon. Although insoluble itself, it is held in solution by carbonic acid. This gas is produced by the decomposition of organic matters, and is one source of the carbonate of lime in the surface well waters. The carbonate of lime is the most common source of the *hardness* of the waters of London. It may be got rid of by Clark's process, which consists in adding lime to the water; the lime combines with the carbonic acid, and throws down a double quantity of carbonate of lime: that is, the carbonate formed, and that held in solution. This process would greatly improve the Thames water. It throws down not only carbonate of lime, but a considerable quantity of organic matter. This plan is carried out most successfully on a large scale at Plumstead. It

was recommended by the Government Commissioners, on account of its "health, comfort, and economy."

2. *Sulphate of Lime* existed in the proportion of from 1 to 15 grains in the gallon. It decomposes in contact with organic matters, and produces sulphuretted hydrogen. Very small quantities of organic matter serve to produce this effect.

3. *Chloride of Sodium* existed in Thames water, in from 1 to 4 grains in the gallon; in deep wells, from 10 to 17 grains; and in surface wells, from 20 to 40 grains. In the Thames it might be the produce of the tide, in the deep wells it was washed out of the chalk; but in the surface wells, where it was most abundant, there could be little doubt that it was derived from the animal and vegetable refuse of the houses through which it percolated. The analyses of above one hundred of these wells showed that they were all equally open to suspicion on this point.

4. *Phosphates and Silica* existed in all the London waters, in small quantities.

5. *Ammonia* also had been detected in small quantities in the Thames, in much larger and more appreciable quantities in the surface wells. This substance was the result of the decomposition of animal matter; and in the surface wells was undoubtedly derived from human excretions.

6. *Nitrates* resulted from the oxidation of the ammonia. They were absent in deep wells; existed only in very small quantities in the Thames, but in large and sometimes even dangerous quantities in surface wells. In one water, examined by Mr. Noad, above 50 grains in the gallon were detected.

The *organic* matters were not injurious when fresh or recent, but they assumed certain conditions of decomposition, which occasionally rendered them deadly. Their influence might be estimated by the case of the Lambeth and Vauxhall Water Company's supply, during the years 1848 and 1854,—two years in which cholera visited London. In 1848, both companies derived their supply of water from the Thames, at Battersea, and both supplied the same district with water, and the houses supplied were equally visited with cholera. But in 1854, the Lambeth Company obtained an improved supply high up the Thames, at Ditton. The consequence was, according to Dr. Snow's calculations, that the deaths amongst the population supplied by the Vauxhall Company, as compared with the Lambeth, was as 7 to 1; according to the most favourable view of the case, as given by Mr. Swain, it was 3½ to 1. There was nothing to account for this difference but the larger quantity of organic impurity in the water supplied by the Vauxhall Company, which still obtained water from the more impure source. The outbreak of cholera in the Golden Square district, in September 1854, was traced to the pump in Broad Street, which was subsequently found to have communicated with the drain of a neighbouring house. Other cases of disease

connected with the contamination of water by organic matter were related.

It appeared, also, that water containing organic matter acted on lead, and thus added another source of poisoning to its own. This had been pointed out by Mr. Noad and Dr. Medlock. The latter chemist held that nitrous acid was formed from the organic matter which, uniting with the lead, formed a quadribasic nitrite of lead. This yielded up its oxide of lead to carbonic acid, forming an insoluble carbonate of lead, leaving the nitrous acid free to act on further quantities of lead. Dr. Medlock explained the action of distilled water on lead as resulting from the water being distilled from water containing organic matter. Water carefully redistilled did not act on lead. But Mr. Faraday had found that water obtained from melting pure ice was the purest water that could be obtained, and it was shown that this water acted on lead. Organic matters in standing water underwent a kind of fermentation, by which carbonic acid, sulphuretted hydrogen, and other gases were got rid of, and nitric acid was formed. The water thus underwent a process of self-purification. This occurred in Thames water, and accounted for the fact that ships were often supplied with water from the Thames below London Bridge. This water was dangerous to drink before or during the fermenting process.

The appreciation of small quantities of organic matters by chemical processes, was a difficult process. During the evaporation of water, the organic matters were dissipated, and not all left in the evaporating basin.

The microscope was an important aid. It detected the nature of organic impurities. These consisted of *dead* and *living* animal and vegetable matters. The dead consisted of the tissues of animals and plants. The source of these impurities could in some instances be made manifest. Such impurities were very manifest in the Thames and surface well waters; scarcely to be detected in the deep well waters. The living matters consisted of plants and animals. Amongst the plants were to be found forms of *Desmidiæ*, *Diatomaceæ*, and *Confervæ*. Some forms of the latter family were especially characteristic of impure waters. One form, the *Calothrix nivea*, was found most abundant in water containing sulphuretted hydrogen. The filaments of microscopic *Fungi* had been found in impure well water. They had been detected in several waters known to have been productive of disease. The lecturer had recorded two instances (*Quarterly Journal of Microscopical Science*, vol. iv. p. 270), and others had been published.

Amongst the living animals, the forms of *Infusoria* were most abundant. These were frequently indicative of the impure condition of water. Eggs of the higher animals were not unfrequently found in the Thames water; and some of these undoubtedly belonged to those forms of *Annulosa*, which find their highest development in the human body.

Many of these forms of animal and vegetable life are not injurious in themselves; but they are most numerous where there is the greatest amount of impurity, and are a measure of the greater or less objectionable nature of a water for drinking purposes. They were not present in water freshly drawn from deep wells.

From these circumstances it was concluded, that the water from deep wells was most desirable and unobjectionable as drinking water; *that the water from surface wells ought under no circumstances to be drunk at all;* and that if Thames water was used, it ought to be filtered, or what is better, *boiled and filtered.* Boiling expelled the carbonic acid from water, and rendered it vapid; but its briskness might be restored by passing it through the gasogene. In the filtration of water various agents may be used, as sand, sponge, charcoal, rock, &c. The most effectual is animal charcoal, which may be introduced into any of the ordinary forms of filter. Dr. Medlock had shown that the addition of iron to water containing organic impurities, precipitated them without rendering the water metallic. Water, which had been filtered in contact with iron twelve months since, was exhibited and compared with water which had not been thus filtered; the latter showed a large quantity of impure vegetable growth, whilst the former was quite pure. Water, which had been obtained from the wells at Watford three years ago, was also exhibited, and showed no signs of vegetation; also water which had undergone "Clark's process," and was equally pure.

[E. L.]

WEEKLY EVENING MEETING,

Friday, February 12.

H.R.H. The Prince Consort, K.G. D.C.L. F.R.S. Vice-Patron, in the Chair.

Professor Faraday, D.C.L. F.R.S.

Remarks on Static Induction.

The object of the speaker was to give to the Members of the Royal Institution a simple reference to the production and nature of the static phenomena of electricity; especially in respect of induction, into which indeed they all resolve themselves. When flannel, shell-lac, metal, and sulphur, are any two of them rubbed together *they* become electrified in the well-known manner; and in such order

that any one of them becomes negative to those which precede it in the list, or positive to those which follow. Thus, metal becomes negative to shell-lac, and positive to sulphur; and as either of these substances can be employed in the investigation of the fundamental principles of induction, this difference is important in some of the methods of examining by the electrometer their temporary or permanent state. If a stick of shell-lac have a flannel cap fitted to one extremity, both being unexcited, and these, either separate or associated, be examined by the gold-leaf electrometer, they will show no signs of electricity. If the cap, grasped by the hand, be turned round on the shell-lac with friction, but left in its place, the associated substances will still show no signs of electricity. If separated, each will show a strongly excited state opposite to that of the other. If one be laid on the cap of the electrometer (the gold leaves of which were 7 inches long and 1½ inches wide, with perfect insulation) it will show a highly excited state; if the other be gradually brought near, and finally placed by the side of the first, all the electric signs will disappear, to reappear when the separation is again produced. The experiment presents a type case of excitation and induction. By the friction together the opposite electricities are excited; they then exist and keep their state by mutual induction; they are perfectly equivalent to each other, and hold their existence by this definite and relative equivalency; for one electricity cannot exist by itself. They show no external signs of electricity whilst the forces are related only to each other, but when the two bodies in which the states are located are separated, then this relation is not exclusive, but by so much as the induction is diminished between the two substances, it is thrown in other directions; as towards the electrometer, or the walls of the room. When one is carried into a separate room, or put into a vessel of conducting matter, then the excited bodies become independent of each other; each has raised up an exactly equal amount of the contrary force by action terminating at a distance, according to the laws of ordinary induction. The power exerted by each excited body in this distant action may be expressed by the term, lines of force. These lines, or the force they represent, are sustained, so long as they are contained in or pass through an insulating medium. They continue, until meeting with conducting matter they evolve the contrary state to that at which they originate, and in the equivalent proportion, and so terminate the insulation; or failing that, they continue their course outwards. If it were possible to place the excited shell-lac in the centre of an almost infinite extent of insulating medium, the lines of force would be as infinitely extended from it. If the power at any section of the whole of the lines of force could be compared with that at any other section, they would be found equal to each other; though one section might be close to the shell-lac, and the other at an infinite distance. If there were no conducting matter at the boundaries of

the insulating space above supposed, the shell-lac could not exist independently in the excited state: it would then keep its lines of force altogether turned upon the body by which it had first been excited, the induction between the two being sustained by their reciprocal action, without which electricity could neither be excited nor exist. Such are some of the consequences which follow inevitably upon the laws of static induction, combined with the law of the conservation of force.

But if this function of induction be so essential to the very existence of electricity in its developed or active state, what is its nature? It acts through distance and across intervening bodies: how are the space and the bodies affected? In all actions at a distance it is most important to ascertain, if possible, what occurs in the intervening medium, or the interposed space; whether the investigation ends in the establishment of a particular process for the particular case, or the reference of the process to any more general mode of action representing all cases of distant action.

Induction acts across any insulating body, whether it be solid, fluid, or gaseous. Common air is concerned in most inductive actions, but being mobile, its particles cannot be retained in a given place, position, or state, so as to allow of close examination. Sulphur and shell-lac are excellent bodies as subjects of investigation, the more especially as their *specific inductive capacity* is about twice that of air; and being solid bodies, their superficial or bounding particles can be thrown into a given state, yet preserved in their place to be examined with the purpose of showing what that state is. If a round plate of metal, 9 inches in diameter, be set up vertically in the air and insulated, and a like plate of good gutta-percha raised on an insulating pillar be placed parallel to and about 9 inches from it; then, upon exciting the gutta-percha, strong induction occurs. The gutta-percha presents the inductric, the copper plate the inducteous, surfaces which limit the field of induction, which field supplies an excellent place for experiment. The gutta-percha should be excited by a piece of close broad cloth, free from loose particles, and all dust, or other sources of convective effects should be avoided. Plates of sulphur, about 3 or 4 inches square, and 1 inch thick, may be employed as the inductive medium, and these having white silk loops introduced into he edges when cast, may, by the further use of white silk slings, be suspended or handled with perfect facility. Some discs of stiff paper, gilt on both sides, being attached at the edge to thin stems of shell-lac, are thus well insulated, and serve either as metallic plates or carriers.

It is almost impossible to take a block of sulphur out of paper, or from off the table without finding it electric; if, however, a small spirit-lamp flame be moved for a moment before its surface at about an inch distance, it will discharge it perfectly. Being then laid on the cap of the electrometer it will probably not cause divergence of

the gold leaves; but the proof that it is in no way excited is not quite secure until a piece of uninsulated tinfoil or metal has been laid loosely on the upper surface. If there be any induction across the sulphur, due to the feeble excitement of the surfaces by opposite electricities, such a process will reveal it: a second application of the flame will remove it entirely. When a plate of sulphur is excited on one side only, its application to the electrometer does not tell at once which is the excited side. With either face upon the cap the charge will be of the same kind, but with the excited side downwards the divergence will be much, and the application of the uninsulated tinfoil to the top surface will cause a moderate diminution, which will return as the tinfoil is removed; whereas, with the excited side upwards, the first divergence of the leaves will be less, and the application of the tinfoil on the top will cause considerable diminution. The approximation of the flame towards the excited side will discharge it entirely. The application near the unexcited side will also seem partly to discharge it, for the effect on the electrometer will be greatly lessened; but the fact is, that the flame will have charged the second surface with the *contrary* electricity. When therefore the originally excited surface is laid down upon the cap of the electrometer, a diminished divergence will be obtained, and it is only by the after application of uninsulated tinfoil upon the upper surface that the full divergence due to the lower surface is obtained.

Being aware of these points, which are necessary to safe manipulation, and proceeding to work with a plate of sulphur in the field of induction before described, the following results are obtained: A piece of uncharged sulphur being placed in the induction field parallel of course to the gutta-percha and copper-plates, and retained there, even for several minutes, provided all be dry and free from dust and small particles, when taken out and examined by the electrometer, either without or with the application of the superposed tinfoil, is found without any charge. The gilt plate carrier before described, if introduced in the same position and then withdrawn is found entirely free of charge. If the sulphur plate be in place, and then the carrier be introduced and made to touch the face of the sulphur, then separated a small space from it, and brought away and examined, it is found without any charge; and that whether applied to either one side or the other of the block of sulphur. So that any of these bodies, which may have been thrown into a polarized or peculiar condition whilst under the induction, must have lost that state entirely when removed from the induction, and have resumed their natural condition. Assuming, however, that the sulphur had become electrically polarized in the direction of the lines of induction, and that therefore whilst in the field one face was positive and the other negative, the mere touching of two or three points by the gold-leaf carrier would be utterly inefficient in bringing any sensible portion of this charge or state away; for though metal can

, *conduction contact* with the surface particles of a mass of g matter, and can take up the state of that surface, it is / real contact that this can be done. Therefore the two of a block of sulphur were gilt by the application of gold leaf on a thin layer of varnish, and when the varnish was quite dry and hard this block was experimented with. Being introduced into the induction field for a time and then brought away, it was found free from charge on both its surfaces; being again introduced, and the carrier placed between it and either the gutta-percha or the copper-plate, but not touching these or the sulphur, the carrier when brought away showed no trace of electricity. The carrier being again introduced at the inductive or gutta-percha side, made to touch the gilt surface of the sulphur on that side, separated a little way and then brought out to be examined, gave a positive charge to the electrometer: when it was taken to the other side of the sulphur and applied in the same manner, it brought away a negative charge. Thus showing, that whilst the sulphur was under induction, the side of it towards the negative gutta-percha was in the positive state, and the side towards the positive inducteous surface of copper bounding the extent of the induction field, was in the negative state. Thus the dielectric sulphur whilst under induction is in a constrained polar electrical state, from which it *instantly* falls into an indifferent or natural condition the moment the induction ceases, either by the removal of the sulphur or the gutta-percha. That this return action is due to an electrical tension *within* the mass, sustained while the act of induction continues, is evident by this, that if the carrier be applied two or three times alternately to the two faces, so as to discharge in part the electricity they show under the induction, then on removing the sulphur from the induction field it returns, not merely to neutrality or indifference, but the surfaces assume the opposite states to what they had before; a necessary consequence of the return of the mass of inner particles to or towards their original condition.

The same result may be obtained, though not so perfectly, without the use of any coatings. Having the uncoated sulphur in its place, put the small spirit lamp between it and the copper-plate; bring up the excited gutta-percha to its place, remove the spirit-lamp flame, and then the gutta-percha, and finally, examine the sulphur: the surface towards the flame, and *that only*, will be charged—its state will be found to be positive, just like the same side of the gilt sulphur which had been touched two or three times by the carrier. During the induction, the mass of the sulphur had been polarized; the anterior face had become positive; the posterior had become negative; the flame had discharged the negative state of the latter; and then, on relieving the sulphur from the induction, the return of the polarity to the normal condition had also returned the anterior face to its proper and unchanged state, but had caused the other, which had been discharged of its tem-

porary negative state whilst under induction, now to assume the positive condition. It would be of no use trying the flame on the other side of the sulphur plate, as then its action would be to discharge the gutta-percha, and destroy the induction altogether.

When several plates were placed in the inductive field apart from each other, subject to one common act of induction, and examined in the same manner, each was found to have the same state as the single plate described. It is well known that if several metallic plates were hung up in like manner, the same results would be obtained. From these and such experiments, the speaker took occasion to support that view of induction which he put forth twenty years ago,* which consists in viewing insulators as aggregates of particles, each of which conducts within itself, but does not conduct to its neighbours, and induction as the polarization of all those particles concerned in the electric relation of the inductric and inducteous surfaces; and stated, that as yet he had not found any facts opposed to that view. He referred to specific inductive capacity, now so singularly confirmed by researches into the action of submarine electro-telegraphic cables, as confirming these views; and also to the analogy of the tourmaline, whilst rising and falling in temperature, to a bar of solid insulating matter, passing into and out of the inductive state.

[M. F.]

WEEKLY EVENING MEETING,

Friday, February 19.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

EDMUND BECKETT DENISON, Esq. M.A. Q.C. M.R.I.

On some of the Improvements in Locks since the Great Exhibition of 1851.

IT is impossible to give an intelligible abstract of the mechanical details of this lecture without drawings; but the substance of it will be found, among other matters relating to locks, in the second edition of Mr. Denison's book on *Clocks and Locks*,† reprinted, with additions, from the *Encyclopædia Britannica*.

He proposed to take up the subject where it was left by the late

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* Phil. Trans., 1837.
† Published by A. and C. Black, Edinburgh; and sold at Dent's, 61, Strand.

Professor Cowper, in a lecture here in 1852. Mr. Cowper had been one of the arbitrators in the case of Hobbs *versus* Bramah, and he explained in that lecture the mode in which both the Bramah and Chubb locks had been picked by Mr. Hobbs in 1851; which applied equally to every English lock then in existence, of a higher order than the common warded locks, which had long been known to possess no security at all.

Mr. Denison said he believed there was still a notion prevailing among persons who ought to know better, that the picking of locks by this method required singular skill and dexterity, such as need not be feared from ordinary lock-pickers. That was quite a mistake, now that the method is so well known to everybody in the lock trade, and to everybody who takes the trouble to look into any modern book on locks. The long delay of the thieves who opened the box with a Chubb lock on the South-Eastern Railway, in the gold-dust robbery, only proved that they were grossly ignorant of their business. Any moderately good hand, among not merely Mr. Hobbs's but Mr. Chubb's own workmen, would have opened the box, and shut it up again, between London and Reigate. Indeed, in a trial before Lord Campbell, a few years ago, one of Mr. Chubb's men confessed that the picking of one of his locks, or of any others then known to him, was merely a question of time; and Mr. Hobbs has several times said in public, that he is acquainted with persons, both in the trade and out of it, who can pick locks quicker than he can, now that the proper way of doing it is known.

Moreover, the time required to pick the best locks of the usual construction is really very small. Mr. Denison said he had seen one of the newest Bramah locks, with eight sliders, or eight slits in the key, picked in less than four minutes; and it is part of the regular business of Mr. Hobbs's shop in Cheapside, to send men to open Chubb and Bramah, and other locks of which the keys have been lost, or which have got out of order.

For this and other reasons, more fully explained in the book referred to, the Bramah or Mordan lock, which has not been at all improved since 1851, has no longer any pretence to be reckoned among the secure class of locks, but ought to rank with others—if not below them,—which are now sold for much less, such as Tucker's, Parnell's, and Hobbs's cheap locks, even those which have not the addition to be mentioned presently. One of the good effects of the exposure and defeat of our best locks has been the invention of a greater variety of really different locks in the last six years, than in the previous sixty, or six hundred. The competition thus arising, and especially the establishment of a lock factory by Mr. Hobbs himself, where locks are made by machinery (like Colt's revolvers and the American clocks), has effected a very great reduction in their price. The common three-inch drawer locks, with four tumblers,

equal to the best locks of 1851, are now sold by him at 27*s.* a dozen, and by Mr. Tucker, and perhaps other makers, at about the same price. It may be as well to mention that the piece called the "detector," by which the Chubb lock gained so much of its celebrity, is of no use whatever towards the prevention of picking by the method which is now generally connected with Mr. Hobbs's name; though it is in reality much older, and been actually published in a former edition of the *Encyclopædia Britannica*, 30 years ago, as applicable to the Bramah lock as first made, without false notches; only no one had ever thought of applying it to tumbler locks, which were invariably made without false notches before 1851, with the single exception of Strutt's lock, which was invented in 1819, but never came into general use.

It should be observed that the mere variety of locks of different constructions, and the same outward aspect, is to a certain extent a source of security. Formerly, a thief knew almost by looking at the key-hole, of whose make and of what construction a lock was; but there are now so many locks and keys, all of the same general appearance, that you can no longer tell by the mere look of the outside what kind of machinery you have to deal with inside; any more than Mr. Hobbs knew, when he accepted a challenge to pick one of Mr. Cotterill's locks, that it contained an additional contrivance not to be found in those locks as generally made and sold, and therefore of course he failed, though the Cotterill locks commonly sold are quite as easy to pick as Bramah's, and in just the same way.

The principle of all locks above the rank of the common warded locks, whatever may be the details of their arrangement, is this:—There are a number of similar pieces (which may have the name of tumblers, levers, sliders, discs, rings, or pins, according to circumstances), each with a notch in it at a different place; and until all these notches are brought together into one given position, the pieces in question, or some of them, prevent the passage of another piece in the lock, on which its opening depends. Mr. Denison exhibited a model, made not to resemble any particular lock, but to illustrate this general principle of them all; and he showed on it the application of the "tentative" mode of picking by applying pressure to the bolt, and then gently moving each of the tumblers or sliders, &c., in succession, on which any pressure is felt, until all their notches come under the piece which has to enter them, and then the bolt yields to the pressure, and goes back

As soon as the vincibility of all the best English locks by this method had been demonstrated in 1851, the makers of tumbler locks began re-inventing several old contrivances, and especially the false notches of Strutt's lock of 1819, or it may be said, of Bramah's lock of 1817; for all the good Bramah locks (including the very one on which Mr. Hobbs won the 200 guineas) had been made with false notches since that time; and tumbler

locks in America had also had them, as well as other contrivances since introduced here, but without defeating the art of the lock-picker. False notches do undoubtedly increase the difficulty of picking, and the time required for doing it; but the facts already mentioned are quite sufficient to show that they do nothing more. Of course that is worth something; but it is very far short of restoring the Chubb and Bramah locks, for instance, to the position which they enjoyed while the tentative method of picking was unknown, and when they really were impregnable by any known mode of manipulation.

The first invention which really defeated this process of lock-picking was appropriately enough Mr. Hobbs's own. What he calls his "protector," or moveable stump lock, is so made, that as soon as you try to make the bolt press upon the tumblers, the pressure is taken off them altogether, and transferred to a fixed pin in the lock, which would prevent it from being opened in that state of things, even if all the tumblers were then raised to the proper height. This is done by the moveable stump, for a description of which we must refer to the book above-mentioned, or to the *Rudimentary Treatise on Locks*, in Weale's Series. This invention, in its present form, is perfectly effectual against any mode of picking yet known. At any rate, a challenge to attempt it was refused by that same Mr. Goater, who confessed at the trial above referred to, that he could pick the locks of his own master, Mr. Chubb, and pretended to be able to pick any others as well, and had really picked one of Hobbs's locks as they were made at first. This "protector" lock, it should be observed, is quite distinct from the great American lock with a changeable key, also made by Mr. Hobbs here, but invented by Day and Newell, of New York, a far more complicated and expensive machine, and with the disadvantage of requiring a very large key, and apparently not more real security than the "protector" lock, except so far as the power of changing the lock by re-arranging the "bits" of the key may be supposed to increase its security by guarding against the risk of an impression being taken from the key; and the lock is also not without some special risks of its own, which might have remarkably unpleasant consequences, if the key got into the hands of a mischievous person, either while the lock is open or shut.

A variety of inventions are described in the above-mentioned book on *Clocks and Locks*, and in the large volume on locks, published by Mr. Price, of Wolverhampton, all aiming at the same object as the moveable stump, but very few of them doing it successfully. Revolving curtains, and barrels, and "detention catches," and "self-acting," and "double-action levers," and a variety of other recent inventions of old and new things, may be dismissed at once with the same remark as the false notches; viz., that they make a lock more troublesome to pick, but they do no more; and they are all *only* more complicated contrivances for doing that incompletely

which is done completely and with great simplicity by the moveable stump of the "protector lock."

The only inventions of this class which Mr. Denison thought deserving of special notice, were a series of locks by Mr. Tucker, of Fleet Street, of which specimens were exhibited, with a large model of one of them, as of the other locks described in the lecture. They have also the merit of being simple and cheap, and unlikely to get out of order, and if not equal to Mr. Hobbs's in security, they would baffle any but a very first-rate hand at lock-picking, and are certainly superior to several more expensive locks which profess to defeat the tentative mode of picking.

The lecturer also described and exhibited a lock of an entirely different construction, invented by himself. It is not intended for furniture and small work, but for doors of safes, prisons, and other places where a lock of great strength as well as security is required. A description of it will be found in all the three books above-mentioned, and it is the only English lock to which the merit of security against any known method of picking is ascribed by Mr. Hobbs, in the *Rudimentary Treatise on Locks.* Its peculiarities and advantages are, besides the important one already mentioned: First, that a large and strong lock requires only a very small key. (2.) It requires no key to lock it, but merely the turning of a handle, the key being required for unlocking only. This obviates the necessity for leaving your keys in the hands of other persons, if you are only present when you want to open your door or safe, and also removes the temptation to leave them in the lock, which affords great facilities for having impressions taken from them. (3.) The key-hole is so small that no instrument strong enough to force the lock can be got in. (4.) The key-hole is kept closed by a spring plate or curtain, which is pushed in by the key; and when the lock is open no instrument whatever can be got into the key-hole to explore the lock. (5.) This curtain also keeps out dirt and damp air, which frequently cause locks to get out of order, or at any rate to want cleaning. (6.) There are no tumbler springs, and so the tumblers can neither fail from the springs breaking, nor can they stick together; both of which things not unfrequently happen in tumbler locks with springs, and without separating plates. (7.) The moving pieces in the lock are as few as possible, being in fact none but the bolt, the tumblers, and the curtain. (8.) Hence also the lock is easy to make, and cheap, and requires no fine work to prevent friction of the parts, and make it move easily. It is not patented; the inventor being one of that increasing number of persons who think that patents are an obstruction to science, and waste more money than they gain for real inventors, whatever they may do for patent-agents and patent-buyers. One of the locks exhibited on this construction was made by Mr. Chubb; and Mr. Hobbs also applies them to safes, &c. when ordered, though they are not yet made for sale.

The spring-curtain of this lock may be adapted to almost any other, and is particularly recommended for street-door or "latch" locks, as they are called, which are very liable in such towns as London to be spoiled, or put out of order, by the action of the air and dirt upon them. Mr. Hobbs adds this curtain to his latch locks for a trifling extra charge, the cost of making it being insignificant; and it supersedes the necessity for an external "scutcheon" on the key-hole, which seldom keeps long in action, and is not so effectual as this self-acting curtain.

Mr. Denison also exhibited three small bells, made by Mr. Mears: one of the same metal as the great bell of Westminster which he is now re-casting; another, with the addition of as much silver as would amount to 1 cwt. and cost £500 in a 16-ton bell; and the third with rather more. These bells clearly bore out the statement made in the lecture on bells last year, that the tone would not be improved by adding silver, of which also no trace has been found in any old bell-metal that has been analyzed.

[E. B. D.]

WEEKLY EVENING MEETING,

Friday, February 26.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

REV. BADEN POWELL, M.A. F.R.S. F.G.S. F.R.A.S.

SAVILIAN PROF. OF GEOMETRY, OXFORD.

On Rotatory Stability; and its Applications to Astronomical Observations on board Ships.

THE subject of rotatory motion, especially when taking place under those combinations which are presented in the gyroscope, or free balanced revolver, has attracted much attention at the present day; and though the primary mechanical principles bearing upon it had been long since understood and acknowledged in *theory*, yet the practical results to which they might lead had been so little considered, that when first tangibly exhibited they excited unbounded surprise.

Even some scientific persons were at a loss to account for them, or sceptical as to their real nature; especially when they witnessed the wonderful results obtained by M. Foucault, apparently subverting the laws of equilibrium, and looking more like magic or legerdemain than sober philosophical experiments. Yet while

these results excited so much wonder when exhibited with a refined apparatus, and in a scientific form, it was forgotten how perfectly similar, and equally paradoxical in its nature, is the common and familiar result of a top sustained by the mere act of spinning in a position from which it directly falls when the rotation ceases.

Some results of this kind, which had then become known, were on a former occasion brought under the notice of the members of the Royal Institution (March 3, 1854*). On that occasion, therefore, the discussion was confined to the principle of "composition of rotations," and those applications of it which had been found in certain rotatory phenomena of projectiles, illustrated by the gyroscope in its several earlier forms as successively modified by Bonenberger, Atkinson, Fessel, and Wheatstone, showing the identity of these results on a small scale with the grand cosmical phenomenon of the precession of equinoxes. Since the date of that lecture, the striking results produced by merely carrying out the same principles, and applying the gyroscope to demonstrate directly the fact of the earth's rotation, as well as under other conditions to point to the poles, by M. Foucault, have become familiarly known. It is, however, an act of justice to mention that the former result (the proof of the earth's rotation) was eighteen years before fully pointed out by Mr. Sang,† of Edinburgh, and only not practically accomplished from the expense of the necessary apparatus. In recurring to the subject on the present occasion, the object is to explain another application of the same principles, like the former very obvious *when once disclosed;* but which nevertheless remained unknown and unthought of until it was pointed out and actually effected by the inventions of Prof. C. P. Smyth : the use of *rotatory apparatus for giving an invariable plane or platform for astronomical instruments used at sea.* To render the subject intelligible it is necessary to recur to two simple first principles in dynamics, which, when distinctly apprehended, give the clue to the whole of the applications.

The first of these is the tendency of a body in rotation to retain that rotation in the same plane, when perfectly balanced, irrespective of the motion of external objects, which is termed "the fixity of the plane of rotation."

The second is "the composition of rotatory motion :" or that when a force is impressed on a body in rotation, it does not show itself directly, but is *compounded* with the first motion ; so that the rotation takes an intermediate direction, or the axis shifts its position in space. This being the cause of the motion of the earth's axis, giving rise to the precession of equinoxes, it is called generally a "precessional motion."

* Proceedings of the Royal Institution, Vol. i., p. 393.

† See Edinburgh New Phil. Journal, Oct. 1836 and 1837 ; and Proceedings of the Royal Scottish Society of Arts, 1856.

The principle of fixity of the plane of rotation had been universally recognised in theory; and it could not have been doubted that in proportion to the momentum acquired by giving immense velocity to the rotating mass, this constancy would be more vigorously displayed; yet perhaps few were prepared for the actual result as exhibited by M. Foucault: even when the principle was acknowledged, nothing could seem more astonishing than the obstinate resistance of the disk to any inclination from its original plane of rotation; which no ordinary degree of force would overcome. This principle is that chiefly referred to in the inventions about to be described, where the effect depends essentially on the great amount of resistance thus offered to any angular motion impressed by an extraneous cause on a perfectly balanced revolving heavy disk.

When we consider the vast amount of precautions taken by astronomers for securing the *stability* of their instruments, and the careful plans adopted for guarding against every imaginable cause of disturbance on land, it may seem surprising that even any attempt should be made to carry on such operations at sea. Yet it is a matter of necessity: some observations must be made for determining the place of the ship, on which its safety depends; and other cases often occur when phenomena of great value to astronomy,—the science without which the ship could not be navigated,—are required to be observed at sea; or may perhaps only be visible at positions out on the ocean. The most important of these observations are those of the *altitudes* of the heavenly bodies, on which depends both the determination of the *latitude*, and the correction of *time* essential to finding the *longitude;* and for this purpose there is a necessity for a well defined horizon, which it is often impossible to obtain from the state of the atmosphere in its lower parts, though the sun or star can be distinctly seen above, and this more especially at night; yet the safety of the ship may essentially depend on such an observation.

Hence various plans have been resorted to for obtaining an *artificial* horizon. Simple reflection from the surface of a liquid can hardly ever be practicable, on account of the motion of the ship, though it is the usual substitute on land; by the reflected image, seen as much below the true horizon as the object is above it.

The most celebrated attempt to substitute some other principle, was an application of *rotatory motion*, devised by the late Mr. Troughton, in 1820. It consists in causing a disk, truly balanced on a fixed pivot, to spin round with great velocity, so as to keep up its motion during the time required for an observation, known by the name of "Troughton's top." The disk carries a plane reflector on its upper surface; and being a cylinder hollowed out at its lower end, and the point of support within, the centre of gravity is thrown below, so that it is in stable equilibrium when at rest. The

velocity is communicated by a separate train of wheels, from which it can be instantly detached. Thus from the principle *of fixity of the plane of rotation*, it was expected that the reflecting surface would preserve its level, notwithstanding the motion of the ship.

It may be proper here to observe, that simple as this idea was, the practical application of it involved several instances of those mechanical resources which its inventor could so readily supply. One of these (of importance in all similar instruments) is the means of obviating the unavoidable inequalities of density in different parts of the most accurately cast metallic disk, as well as defects in accuracy of form, which affect the *perfect* equilibrium. These were remedied by plugs, or small pieces of metal, screwed more or less deeply into holes made for them at opposite parts of the circumference, *three* vertically, and *three* horizontally placed, by which, in successive trials, a complete compensation was effected.

The method was, however, found practically to fail; and the failure has been since traced to another mechanical principle. The pivot partakes in the irregular motion of the ship. When the disk is *not revolving*, this motion is in turn communicated to the disk; and the centre of gravity being below,—the very circumstance which gives it stability on land—causes it to acquire an oscillatory movement. When in *rotation*, this does not show itself directly, but is compounded with the rotation, and causes a *precessional motion*, which is fatal to its use as a *horizontal* reflector.

Hence, if the centre of gravity *coincided* with the point of support, as would be most readily done by suspending the revolving disk in gymbals in the manner of Bonenberger's machine, this cause of irregularity would be avoided. By this means it would preserve *its original level*; but this would not necessarily nor usually be the *true horizontal* level.

To obtain the *true horizontal point* another contrivance is necessary: this (as not being connected with rotatory motion) can here only be briefly adverted to. It consists essentially in the bubble of a spirit-level reflected from a diagonal mirror above it, seen through a collimating lens, so adjusted that in one inclination the apparent position of the bubble coincides accurately with the true horizontal direction: whence it is easily demonstrated that in every other inclination within certain small limits, the apparent place of the bubble will deviate from its former place by the same angle as that by which the base is inclined, or will in all positions give the true horizontal direction: the level-tube being bent into a small arc of a circle, whose radius is equal to the principal focal length of the lens.*

But for other classes of observations on board ship which involve

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* This contrivance has been fully described in the Notices of the Royal Astronomical Society, Vol. xviii., p. 65. Jan. 1858.

the use of *the telescope*, especially those requiring one of considerable power, the same requisite of invariable stability of direction is yet more indispensable, but hitherto unattained.

One of the most important desiderata of nautical astronomy has always been the means of observing at sea the eclipses of Jupiter's satellites,—so frequently recurring and affording so simple and direct a means of obtaining the longitude. The same want exists also with respect to a variety of other astronomical observations which it is often desirable, if not necessary, to make at sea. Accordingly, this object has engaged the attention of many inventors of schemes for supporting the telescope, and the observer with it, so as to be free from the motion of the ship.

In general, to procure stability on ship-board, it seemed an obvious recource simply to *suspend* any object which it was desired to keep steady by cords from a fixed point in the vessel. But a body thus suspended is like a *plumb line*, when the point of support is itself set in motion: it acquires a part of that motion and becomes *a pendulum;* and it oscillates more irregularly and violently from the accumulation of motions impressed upon it continually by every fresh motion of the ship. The case is the same as that just considered in Troughton's top.

There is, indeed (as Prof. Smyth remarks) a *semblance* of steadiness in that (for example) articles keep their places on a table so suspended; a glass of water placed on it does not spill: but this is only a case of the same kind as when an object so suspended and whirled round, is retained in its place from centrifugal force; its surface keeps perpendicular to the string, not parallel to the horizon. Nairne's or Irwin's "Marine Chair," for carrying the observer and his telescope, was simply an application of this principle. It was tried on board ship, especially in a voyage to the West Indies, by the late Dr. Maskelyne: and though somewhat prematurely rewarded by the Government, was found not to answer: though no one seemed fully aware of the cause of its failure, till Sir J. Herschel (in the *Admiralty Manual*) pointed out the principle just stated, and showed that this free suspension must tend to perpetuate disturbances rather than *destroy* them.

In these cases the centre of gravity is *below;* this is what constitutes the table or chair, a pendulum. If it were suspended in gymbals, so that the centre of gravity should be at the point of suspension, the tendency to oscillate from this cause would be overcome. But still any slight cause might disturb the level: there would be no principle of permanent stability.

Thus, to produce this desired stability for a plane or stand on which the telescope is to be rested, we must have recourse to the *free revolving disk accurately balanced within gymbals, on its centre of gravity.* The balancing must be perfected by means of the adjustable plugs before-mentioned both in the disk, and in the gymbal frames; the pivots of the gymbals must be of

perfect workmanship, to turn with the least possible friction, yet without looseness or displacement. An immense rotatory velocity must be communicated to the disk, by machinery, of which its suspension must be quite independent, so that the moving power can be instantaneously withdrawn. All these conditions are fulfilled in the form of the machine which, after repeated trials, has been adopted by Prof. Smyth, exhibited by him at the Paris Exhibition 1855, and successfully tried on board Mr. Stephenson's yacht *Titania*, on his voyage to Teneriffe, in 1856.*

To explain the principle of its action, we must consider the general conditions of the case.

In the first instance, it may be necessary to bear in mind that a telescope pointed to a star may undergo a motion of *translation* to any extent and in any direction, provided it retain strict *parallelism* to its original position, that is, remain pointed to exactly the *same altitude* and *azimuth*, and it will retain the object in its field just as if it were absolutely fixed. Thus, if a ship be going in a perfectly *straight* course on a perfectly *level* sea, or even if its motion on the waves were confined to one *straight* direction up and down, this *parallelism* might be retained. But this is not the actual case; even if the course be perfectly straight the pitching and rolling take place by *angular motions*, in planes respectively parallel and perpendicular to the length of the ship. The slightest *angular* motion is destructive to the *parallelism* of the telescope. It is against this, therefore, that it is our object to guard.

The grand principle of *fixity of the plane of free rotation*, is that which enables the revolving disk to retain parallelism to its original plane, however the external frame or pivots supporting the whole be moved. From this principle the revolving disk resists all angular change of position *in directions perpendicular to its plane:* but it offers no resistance to any motion *in that plane.* Thus, if set spinning horizontally, it will resist all angular motion impressed in a vertical plane, either by the *pitching* of the ship in the direction of its *length*, or its *rolling* in the direction of its *breadth:* but it will not resist *lateral* motion in a horizontal plane, such as any shifting of the ship's head towards the points of the compass.

Thus a free revolving disk in gymbals externally turning on pivots horizontally resting on supports fixed to the deck, will suffice to preserve the telescope from all deviation due to pitching and rolling. The addition of another disk, freely revolving in a *vertical plane*, whose external pivots turn vertically in a frame attached to the top of the former internal frame, the upper pivot projecting through it, and carrying a small platform for the telescope, and the

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* See a paper "On the angular disturbances of Ships, &c.," by Prof. C. P. Smyth, F.R.S. Trans. of Royal Scottish Society of Arts, Vol. iv., Part 4. 1857. Also Astron. Society Notices, Vol. xvii., p. 36.

whole, of course, balanced below, will preserve the telescope from any *lateral* deviations of the ship. And the combination of the two will give a plane retaining its parallelism against all three causes of disturbances. But under favourable circumstances this last cause of disturbance is but small: so that this addition may be often of little importance.

The invariable platform of this revolving apparatus may then equally be applied to support either a *telescope*, or the *artificial horizon* before mentioned, whether simply or in conjunction with the sextant, or reflecting circle. By a mere enlargement of the *scale* of the machine, the same stand which carries the telescope might be made to carry the observer also, which would be a material convenience for any nice observations.

But the essential condition is the preservation of *perfect equilibrium* about the centre of motion. Now if this were secured by proper compensation for the observer in *one* position, the slightest change of position on his part would vitiate that arrangement.

The observer, instead of being an *extraneous* source of disturbance incapable of producing any impression on the balanced and revolving system, now becomes *a part of it*, and thus impresses upon it a fresh motion arising from every slight change of posture, which alters the exact position of the centre of gravity of the whole. This effect, however, would not manifest itself *directly*, but being *compounded* with the *rotation*, would show itself in a *precessional* motion, fatal to the stability of the direction of the telescope.

The case is not one of great importance practically, though interesting theoretically. A modified arrangement to meet it has been devised by Prof. Smyth, which will be understood by the following considerations:—

Two free revolvers turning horizontally are placed with the lines of the pivots of their *interior* rings horizontally in directions (P and Q) at right angles to each other, (which we will call the disks p and q respectively,) in a common exterior frame, itself turning on pivots in the direction (Q) in a third exterior frame, whose pivots are on fixed supports in the direction (P).

Now supposing the whole to remain perfectly balanced, if any angular motion in a vertical plane be communicated to the fixed supports, tending to turn the whole about (P), then the revolving disk (q), in order to retain its own parallelism, will cause *both the outer and inner general frames* together to retain their parallelism, by turning about (P) through an angle equal and opposite to that through which the supports are moved; while the disk (p) simply retains its position along with the frames.

An angular motion given to the supports about (Q) will cause the outer frame to move through that angular space. But the revolving disk (p), in order to retain its parallelism, will cause the *inner general frame* to move through an angle equal and opposite to that of the supports about (Q), and the disk (q) will simply *retain its* position.

Now let us suppose that the observer (carried on the inner frame), by some slight movement, displaces the centre of gravity of the whole; or what is the same thing, let us suppose a small weight added to one end of the frame,—suppose, so as to give a tendency to turn about (P), then the disk (p) simply retains its position, but the disk (q) is affected—not, however, directly; but the effect being *compounded* with the rotation, gives a *precessional motion* to that particular disk, which merely causes it to turn on the pivots of its own inner ring. If the small weight were placed so as to produce motion about (Q), precisely the same effect would take place on the disk (p), while (q) would retain its position.

In one word, the precessional disturbance is transferred from the general frame, to the particular revolving disk.

Thus on the whole, as Prof. Smyth observes, "If any want of balance is produced on the frame, it is not immediately altered thereby, the first effect being only to make the appropriate wheel turn on its gymbal pivots. If, too, the wheel is heavy and the speed great, this precessional turning will be so slow that there will be time for an attendant to press in the opposite direction on the general frame, and correct the effect of the observer's want of balance, before the plane of rotation coincides with the direction of the disturbing force, when the virtue of resistance ceases."

To strengthen the effect, and at the same time to give a more convenient balance to the whole, the disks (p) and (q) respectively, are reinforced each by a corresponding revolver at the other end of the same diameter of the frame, and similarly mounted in the inner general frame.

Thus all angular disturbance in *vertical* planes being got rid of, it only remains, on the principle before described, to add a fifth revolver in the middle, in a vertical plane, to counteract angular *horizontal* motion. The inner general frame supports a *vertical* frame, which carries this last revolver. Thus we have the construction of "the compound precessional free revolver stand," as the inventor designates it. But he has found the simple form so effective by itself in the hands of a practised observer, as to render this more complex construction of no immediate necessity.

To complete the whole, Prof. Smyth has carefully investigated the best form of a train of wheels for communicating the rotatory motion: and has also considered the question of the best moving power to be used; which he finds, after many trials, to be that of *water;* which is best brought to bear by a peculiarly beautiful and simple form of the *turbine.*

Thus taking a summary view of the whole subject;—by direct consequence, from the simplest acknowledged mechanical principles, the gyroscope, when its equilibrium is slightly disturbed, demonstrates the precession of equinoxes; explains the boomerang;—and sustains itself in the air against gravitation. When its equilibrium is undisturbed it exhibits to the eye the actual rotation

of the earth; and when restricted to one plane it acts as a magnetic needle without magnetism, or spontaneously rotates in parallelism with the earth. To these remarkable, diversified, and somewhat paradoxical applications, we have now added another of far higher utility, that it gives perfect stability for the nicest astronomical observations on board a ship, pitching and tossing with every wave and gust of wind.

[Besides models, illustrative of the principle, the actual instruments were exhibited by Prof. C. P. Smyth.]

[B. P.]

GENERAL MONTHLY MEETING,

Monday, March 1.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Edward Levi Ames, M.A.
Herbert Barnard, Esq.
George Bishop, jun., Esq.
John Ashton Bostock, Esq.
S. M. Boulderson, Esq.
Edward Hay Currie, Esq.
William Day, Esq.
Alfred Hamilton, Esq.
Joseph Haynes, Esq.
Stanley Haynes, Esq.
Alfred Gutteres Henriques, Esq.
Thomas Hyde Hills, Esq.
William Longman, Esq.
Mrs. Lyon Playfair.
Sir James P. Kay Shuttleworth, Bart.
William Southey, jun., Esq.
William T. H. Strange, Esq.
Robert Tait, Esq.
Matthew Uzielli, Esq.

were duly *elected* Members of the Royal Institution.

William Augustus Hillman, Esq. F.R.C.S.
John Leighton, jun. Esq. F.S.A.

were *admitted* Members of the Royal Institution.

The Secretary announced that the following Arrangements had been made for the Lectures after Easter:—

Nine Lectures on the HISTORY OF ITALY DURING THE MIDDLE AGES, by JAMES PHILIP LACAITA, Esq. LL.D.

Three Lectures (*in continuation*) on HEAT, CONSIDERED AS A MODE OF MOTION, by JOHN TYNDALL, Esq. F.R.S. Professor of Natural Philosophy, R.I.

Eight Lectures on the VEGETABLE KINGDOM, IN ITS RELATIONS TO THE LIFE OF MAN, by DR. EDWIN LANKESTER, F.R.S. F.L.S.

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM

Agricultural Society of England, Royal—Journal, Vol. XVIII., Part 2. 8vo. 1857.
Astronomical Society, Royal—Monthly Notices, Jan. and Feb. 1858.
Bayerische Akademie, Königliche—Abhandlungen, Band VIII. Abth. 1. 4to. 1857.
Rede von Dr. Jolley; und Vortrag von Dr. Von Hermann. 4to. 1857.
Annalen der Sternwarte bei München, Band IX. 8vo. 1857.
Magnetische Ortsbestimmungen, Theil II. 8vo. 1856.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for Feb. 1858. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for Feb. 1858. 4to.
British Architects, Royal Institute of—Proceedings in Feb. 1858. 4to.
De la Rue, Warren, F.R.S. M.R.I.—Index Map of the Ordnance Survey, showing the Path of the Moon's Shadow during the Solar Eclipse, March 15, 1858.
Editors—The Medical Circular for Feb. 1858. 8vo.
The Practical Mechanic's Journal for Feb. 1858. 4to.
The Journal of Gas-Lighting for Feb. 1858. 4to.
The Mechanic's Magazine for Feb. 1858. 8vo.
The Athenæum for Feb. 1858. 4to.
The Engineer for Feb. 1858. fol.
The Artizan for Feb. 1858.
The Illustrated Inventor for Feb. 1858.
Este, M. L. M.D. M.R.I. (the Author)—On the Royal Institution, &c. 8vo. 1810.
On the Drainage of London. 8vo. 1858.
Everest, Rev. R. (the Author)—Sequel to Statistical Details about Lubeck. 8vo. 1858.
Faraday, Professor, D.C.L. F.R.S.—Reports of the Liverpool Compass Committee, with Mr. Airy's Letters thereon. fol. 1857.
Königliche Preussischen Akademie, Berichte, Dec. 1857.
Franklin Institute of Pennsylvania—Journal, Vol. XXXV. No. 1. 8vo. 1858.
Geological Society—Proceedings, Feb. 1858.
Journal, No. 53. 8vo. 1858.
Goldsmid, Aaron Asher, Esq.—A beautiful Manuscript Copy of the Koran, in Arabic. 12mo. 1832.
Graham, George, Esq. (Registrar-General)—Report of the Registrar-General for Feb. 1858. 8vo.
Newton, Messrs.—London Journal (New Series), Feb. 1858. 8vo.
Novello, Mr. (the Publisher)—The Musical Times, for Feb. 1858.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1857. Heft 12. 4to. Gotha, 1857.
Photographic Society—Journal, No. 63. 8vo. 1857.
Prince, C. Leeson, Esq. (the Author)—Summary of a Meteorological Journal, kept at Uckfield, Sussex, in 1857.
Ridley, George, Esq. M.P. M.R.I.—Three Reports on the Use of the Steam Coals of the "Hartley District," of Northumberland, in Marine Boilers. 8vo. 1858.
Royal Society—Report on the Adjudication of the Copley, Rumford, and Royal Medals, and the Appointment of the Bakerian, Croonian, and Fairchild Lectures. 4to. 1834.

Saumarez, Rear-Admiral (the Author)—Introductory Key to the Hieroglyphic Phraseology of the Old Testament. 4to. 1858.
Society of Arts—Journal for Feb. 1858. 8vo.
Taylor, Rev. W. F.R.S. M.R.I.—Specimen of the Great Bell of Westminster ["Big Ben"] now being broken up.
Thomson, Professor W.—Specimen of the Shore-end of the Atlantic Telegraph Wire.
Vereins zur Beförderung der Gewerbfleisses in Preussen—Nov. und Dec. 1857. 4to.
Window, F. R. Esq. (the Author)—On Submarine Electric Telegraphs. 8vo. 1857.

Professor Faraday, *on Static Induction.*

(*Addition to the Report of the* 12*th of February.*)

The inquiries made by some who wish to understand the real force of the test experiments relating to static induction, brought forward on the above date (page 470,) and their consequences in relation to the theory of induction, make me aware that it is necessary to mention certain precautions which I concluded would occur to all interested in the matter: I hope the notice I propose to give here will be sufficient. When metallic coatings or carriers are employed for the purpose of obtaining a knowledge of the state of a layer of insulating particles, as those forming the surface of a plate of sulphur, it is very necessary that they should exist in a plane perpendicular to the lines of the inductive force, and in a field of action where the lines of force are *sensibly equal.* Hence the importance of the dimensions given in the description of the apparatus at page 472 of the report of the evening, when the inductive surfaces are described as 9 inches in diameter, and 9 inches apart. The inductive surface there mentioned is a plane: a ball cannot properly be used for this purpose; for the lines of inductive force originating at it cannot then be perpendicular to the layer of gold-leaf forming the coating of the sulphur. The consequence would be that this layer of gold being virtually extended along the lines of inductive force, *i.e.* having parts nearer to and parts more distant from the inductric, will be polarized according to well-known electrical actions, will have opposite states at those parts, will show these states by a carrier, and will give results not belonging merely to insulating particles in a section across the lines, but chiefly to united conducting particles in a section oblique to or along the lines.

The carrier itself must be perfectly insulated the whole time, or else a case of induction, not including the sulphur, and entirely different to that set out with is established. It must not even *extend by* elongation into parts of the field of induction where the

force differs in degree; or else errors of the same kind as those described with the ball inductric will occur. It should also be so used as to receive no charge by convection. When introduced between the inductric and the sulphur, it is very apt, if the charge be high, or if particles adhere to the inductric, to receive a charge. This is easily tested by introducing the carrier into its place, abstaining from touching the gold-leaf, withdrawing the carrier, and examining it: it is not until this can be done without bringing away any charge that the carrier should be employed to touch the gold-leaf surface, and bring away the indication of its electrical state.

As before said, if when the state of matters is perfect, and no convection interferes, the gilt sulphur be put into its place, left there for a short time, and brought away again, it will be found without *any* charge either of the gold-leaf coating or the sulphur. If it be put into place, the coating next the inductric be uninsulated for a moment only, and the plate brought away, that coating will then appear positive. If it be put into place and the further gold-leaf be uninsulated for a moment, that coating when the plate is brought away will be found negative. These are all well known results, and will always appear if convection and other sources of error be avoided.

22nd March, 1858. M. F.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, March 5.

SIR HENRY HOLLAND, BART. M.D. F.R.S. Vice-President, in the Chair.

PROFESSOR C. PIAZZI SMYTH,

ASTRONOMER-ROYAL FOR SCOTLAND.

Account of the Astronomical Experiment of 1856, *on the Peak of Teneriffe.*

THE object proposed in the astronomical experiment carried out on the Peak of Teneriffe, the year before last, was to ascertain how much telescopic observation can be improved, by eliminating the lower third or fourth part of the atmosphere; in other words, by elevating instruments and observers some 10,000 feet above the sea level.

By such a proceeding, not only was it hoped to rise above the greater part of the clouds, mist, haze, and other aerial impurities, so patent to the eyes of all men,—but also to get rid of certain other sources of optical disturbance, which only manifest themselves to star-gazers, employing telescopes with high magnifying powers. To such, those disturbances are rarely or never absent, even on the finest nights; and they act more and more prejudicially, the larger and more perfect the instruments that may be employed. From this cause it is, that the full magnifying power which a modern telescope is calculated to bear, can very seldom be applied; minute celestial phenomena remain undiscovered; and our opticians must lose heart in carrying on the improvement of object-glass manufacture, if their best performances are to be for ever condemned to have their finer qualities neutralized by the badness of the air in which they are tested.

Here is evidently an evil of no ordinary magnitude, and it has troubled astronomers long. Clearly foreseen and carefully weighed by Sir Isaac Newton, that great philosopher described the nature of the difficulty, and the only means of remedy in 1730, in words as felicitous as comprehensive. "Telescopes," says he, "cannot be so formed as to take away that confusion of rays, which arises from the tremors of the atmosphere. The only remedy is a most

serene and quiet air, such as may perhaps be found on the tops of the highest mountains, above the grosser clouds."

This appears a very simple and probable piece of speculation, yet somehow it dropped out of notice, and never had the seal of practical trial applied to its theoretical prediction, until the late First Lord of the Admiralty, Sir Charles Wood, duly advised by the Astronomer-Royal, Mr. Airy, commissioned me to make the attempt in the summer of 1856, by carrying a large telescope to a considerable height up the flanks of the Peak of Teneriffe.

That mountain was chosen as the most elevated one within reach of a summer expedition, and at the same time of practicable ascent with large instruments. It is situated, moreover, in the middle of the N.E. trade wind region, where the weather is not only more regular than in any other part of the world, but where, *mutatis mutandis*, some pretty certain data as to the climate of the upper atmospheric strata, had been procured from another and grander scientific work, recently performed, also under the Lords of the Admiralty, viz., the remeasurement of La Caille's Arc of the Meridian at the Cape of Good Hope, by their Lordships' southern astronomer, Mr. Maclear.

Further particulars of a practical nature, relative to the character of the ground, as well as the temper and quality of the inhabitants, having been procured from Robert Stephenson, M.P., who had lately visited the island, and whose early experiences on South American cordilleras, had long since led him to look with favour on Newton's mountain method of improving astronomical observation,—the preparations for this novel sort of expedition went on quickly during May and June.

At this stage, so much kindly interest in the attempt was shown in the limited circle of working astronomers, that, beginning with Mr. Airy, I was favoured by them in the course of a few weeks with the loan of many valuable instruments; and finally, Mr. Stephenson, who had already paved the way to the expedition being called into being, tendered the magnificent contribution of no less than the use of his yacht *Titania*, and her able crew. With them, accordingly, we set sail on June 22nd from Cowes; I say we, for I was accompanied by my wife, the best assistant that either an astronomer or any other man can possibly have.

There was still just a trifle of uncertainty spread over our prospects, for some very opposite opinions were in the field, apparently supported by observed facts; and a few voices even loudly proclaimed, that high mountain tops, all the world over, are invariably loaded with clouds and mist and sleet, and tormented for ever with impetuous storms. Yet strong in our own belief, on we went in the swift *Titania*, and arriving in Teneriffe on July the 8th, were at once made free of the island by the liberal and hospitable Spanish authorities; beginning our first ascent six days afterwards, with a long line of mules laden with instruments and baggage.

The morning was desperately cloudy, quite a desponding sort of day; but the angle of ascent in the road was happily most moderate and uniform, so on and up we rode with the greatest facility. A sympiesometer, by Adie of Edinburgh, gave the heights without dismounting. At 3000 feet of altitude, still pacing up a constant slope, the level of the clouds was reached, —those clouds which had made the sky look so unhappy when we were starting from the port of Orotava. A whiff or two of damp mist flew about us for a while, and then we suddenly emerged into clear hot sunshine. From that moment, and hour after hour, as the decreasing column of the sympiesometer chronicled the height ascended, and as we continued toiling up the long slope of the mountain, the sun shone vehemently down upon us from a sky of the purest blue; and never did the clouds below attempt to leave their constant level of 3000 or 4000 feet above the sea. In this brilliant illumination, in the rarefied and arid air, amidst volcanic rocks of grotesque and imposing forms,—in fact, in this most moon-like region we travelled on, until by evening we had reached the top of Mount Guajara, on the southern side of the elevation crater; and thus within 24 days of leaving England, had the satisfaction of bivouacking on the top of a mountain 8900 feet high, and only 28° from the equator; in a calm air, too, with a temperature of 65°, and under a sky undimmed by a single cloud, and gloriously resplendent with stars.

Was not that at once a realisation of Newton's prophetic description, "a serene and quiet air on the top of the highest mountains above the grosser clouds"?—for all this time the lowlands beneath us, and the sea far and wide, were covered in by a broad expanse of mist, whose rollers were driving along under the influence of a violent N.E. wind.

That great plain of vapour floating in mid-air at a height of 4000 feet, was a separater of many things. Beneath, were a moist atmosphere, fruits, and gardens, and the abodes of men; above, an air inconceivably dry, in which the bare bones of the great mountain lay oxidising in all variety of brilliant colours, in the light of the sun by day, and stars innumerable at night.

Below that constant curtain of cloud, were towns and villages, —prisons, theatres and churches many,—above it, save a few goat-herds wandering over the heights with their flocks of Guanche breed, were no traces of human life but in our little astronomical encampment.

Then how truly serene and quiet, and transparent, too, was the air above our 8900 foot elevation; for, on erecting our telescopes, not only was each star, whether high or low, seen with an exquisite little disc and nearly perfect rings, but the space-penetrating power was extended with the same instrument and same eye from the 10th magnitude, at the sea level, to the 14th, on Guajara.

Similar results in their ultimate bearing, followed other obser-

vations, as those of solar and lunar radiation. But time fails me to tell of two months' mountain experience of days, always better for astronomy than in the towns below, and sometimes supremely adapted therefor; and of how, accompanied by two of the sturdy seamen of the *Titania*, we tried our telescopes on the flanks of the Peak itself, at a height of 10,700 feet, ascertained at once the practicability and advisableness of greater heights still, and climbed the culminating point of the mountain 12,198 feet high.

To describe these operations in full, there is now neither time, nor perhaps necessity, as the original observations, with all the numerical and instrumental particulars, have been communicated to the Admiralty, and by them were transmitted to the Royal Society, for publication, in June last; while as to the more popular part of our daily experiences and little personal adventures, should any one care to read them, are they not contained in a little book, recently published by Mr. Lovell Reeve, and illustrated with genuine photo-stereographs?

Such plates being actual reproductions of nature by herself, I may, perhaps, be allowed to call some attention to them, not indeed altogether through means of the book, but by exhibiting, with the assistance of Prof. Tyndall and Mr. De la Rue, magnified optical pictures of some glass copies from the original negatives.

These will be better understood, if attention be turned for a few moments to this large model of the Peak of Teneriffe, and a tract of country about it, sixteen miles square. It has been prepared for this special occasion by the enthusiastic kindness of my friend Mr. James Nasmyth, C.E., long experienced in watching lunar craters in telescopes of his own making; and professionally intimate with metals, fluid and solid, with all the volcanics, indeed as well as the mechanics of the workshop. When he heard of a terrestrial crater, the great crater of Teneriffe, *eight miles* in diameter, he could not restrain his admiration and his zeal; so setting to work with all the map and measurement particulars which I could furnish, he produced the present model, as accurate as the existing state of our knowledge admits.

By a general vertical illumination the colours may be most distinctly seen. The green indicates vegetation, mainly confined to the lower 4000 feet of altitude, or to the region below the clouds. Above them are seen chiefly the colours of the lava rocks; the oldest, light yellow; the most recent, black; and the intermediate, red.

[The first collection of pictures shown illustrated the scenery of the green region on the northern coast; the second had its locale at a height of 8000 feet on Mount Guajara, or the southern wall of the great elevation crater, submarine at the time of its formation. And the third was confined to the eruption crater, or central cone, constituting the so-called Peak of Teneriffe; and

exhibits the phenomena of subaerial volcanic action, at elevations extending from 9000 to 12,200 feet.

Having exhibited the prevailing colours by a vertical light, a ray of electric light was next thrown upon the model, at a low angle, so as to bring out the forms, and especially the angle of slope, of the various cones and craters.]

Studied in this manner, the model will yield so much information, that I will not venture to detain the audience longer, save with a very few words on the social bearing of this astronomical experiment on the Peak of Teneriffe. The claims of science to respect amongst men, for its services in promoting the union of nations and the brotherhood of mankind, have been often dwelt on. Of this admirable and humanizing tendency, is not our experiment on Teneriffe an example, within its little range? See an observer sent out by the English Government, received in a fortified town of the Spaniards, not only without distrust, but as frankly as if one of themselves. And did they suffer by it? We took no notes of their forts and guns, and military array, we applied ourselves to our scientific business alone; and if we have brought away anything more from Teneriffe than what I have already had the honour of describing to you, it is, respect and admiration for the Spanish character; and grand ideas of the results to astronomy as well as some other sciences, if this first experiment, this mere trial of a new method, be annually repeated, and energetically followed out.

[C. P. S.]

WEEKLY EVENING MEETING,

Friday, March 12.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President, in the Chair.

WILLIAM B. CARPENTER, M.D. F.R.S.

On the Lowest (Rhizopod) Type of Animal Life, considered in its relations to Physiology, Zoology, and Geology.

AMONG the unexpected revelations which the modern improved microscope has made to the scientific investigator, there is perhaps none more fertile in interest, than that which relates to the very lowest type of animal existence; from the study of which both the

Physiologist and the Zoologist may draw the most instructive lessons, whilst the Geologist finds in it the key to the existence of various stratified deposits of no mean importance both in extent and thickness.

Though the doctrines of Prof. Ehrenberg as to the complexity of organization possessed by the minutest forms of Animalcules, have now been rejected by the concurrent voice of the most competent observers, working with the best instruments, yet the wonders of animalcular life are not in the least diminished by this repudiation of them. Indeed, as great and small are merely relative terms, it may be questioned whether the marvel of a complex structure comprised within the narrowest space we can conceive, is really so great as that of finding those operations of life which we are accustomed to see carried on by an elaborate apparatus, performed without any instruments whatever;—a little particle of apparently homogeneous jelly changing itself into a greater variety of forms than the fabled Proteus, laying hold of its food without members, swallowing it without a mouth, digesting it without a stomach, appropriating its nutritious material without absorbent vessels or a circulating system, moving from place to place without muscles, feeling (if it has any power to do so) without nerves, multiplying itself without eggs, and not only this, but in many instaneess forming shelly coverings of a symmetry and completeness not surpassed by those of any testaceous animals.

As an example of this type of existence, the *Amœba*, a common inhabitant of fresh waters, may be first selected. This may be described as a minute mass of "sarcode," presenting scarcely any evidence of organisation, even of the simplest kind; for although its superficial layer has a somewhat firmer consistency than the semifluid interior, this differentiation does not proceed to the extent of constituting even a body so simple as the "cell" of physiologists, which consists of a definite membrane investing and limiting its contents. Although at some times shapeless and inert, the Amœba at others is a creature of no inconsiderable activity. Its gelatinous body extends itself into one or more finger-like prolongations; the interior substance transfers itself into one or other of these, distending it until the entire mass is (as it were) carried into it; and then, after a short time, another prolongation is put forth, either in the same or in some different direction, and the body being again absorbed into it, the place of the animal is again changed. When the creature, in the course of its progress, meets with a particle capable of affording it nutriment, its gelatinous body spreads itself over or around this, so as to envelope it completely; and the particle (sometimes animal, sometimes vegetable) thus taken into this extemporised stomach, undergoes a sort of digestion there, the nutrient material being extracted, and any indigestible part making its way to the surface, and being finally (as it were) squeezed out. The Amœba multiplies itself by self-division; and

portions separated from the jelly-like mass, either by cutting or tearing, can develope themselves into independent beings.

Nearly allied to this is another curious organism, on which the attention of many eminent microscopists has been recently fixed. This creature, the *Actinophrys*, has a body whose form is more constantly spherical, but extends its sarcode into radiating filaments of extreme delicacy, which are termed *pseudopodia;* and it is by the agency of these, rather than by the change of place of its whole body (as in Amœba), that it obtains its food. For when any small free-moving animalcule or active spore of a vegetable comes into contact with one of the pseudopodia, this usually retains it by adhesion, and forthwith begins to retract itself; as it shortens, the surrounding filaments also apply themselves to the captive particle, bending their points together so as gradually to enclose it, and themselves retracting until the prey is brought close to the surface of the body. The threads of "sarcode" of which the pseudopodia are composed, not being invested (any more than the sarcode of the body) by any limiting membrane, coalesce with each other and with it; and thus the particle which has been entrapped becomes actually imbedded in the gelatinous mass, and gradually passes towards the central part of it, where its digestible portion undergoes solution, the superficial part of the body with its pseudopodial prolongations in the meantime recovering its previous condition. Any indigestible portion, as the shell of an Entomostracan, or the hard case of a Rotifer, finds its way to the surface of the body, and is extruded from it by a process exactly the converse of that by which it was drawn in.

If, now, it be asked in what consists the peculiar animality of beings thus destitute of every feature that we are accustomed to associate with the idea of an animal,—that is, if it be enquired what are the characters by which they are distinguished from vegetable organisms of equal simplicity,—the physiologist cannot with confidence reply that sufficient evidence is afforded by the movements of the Amœba and Actinophrys; since among the lowest Plants there are many, which, at least in certain stages of their lives, are endowed with yet even greater activity. A more positive and satisfactory distinction lies in the nature of their aliment, and in the method of its introduction. For whilst the *protophyte* obtains the materials of its nutrition from the air and moisture that surround it, and possesses the power of detaching oxygen, hydrogen, carbon, and nitrogen from their previous binary compounds, and of uniting them into ternary and quarternary organic compounds (chlorophyll, starch, albumen, &c.), the simplest *protozoon*, in common with the highest members of the animal kingdom, seems utterly destitute of any such power, and depends for its support upon organic substances previously elaborated by other living beings. Further, whilst the protophyte obtains its nutriment by simple imbibition, the protozoon, though destitute of any proper stomach,

extemporizes, as it were, a stomach for itself in the substance of its body, into which it ingests the solid particles that constitute its food, and within which it subjects them to a regular process of digestion. Hence these simplest members of the two kingdoms, which can scarcely be distinguished from each other by any *structural* characters, seem to be *physiologically* separable by the mode in which they perform those actions wherein their life most essentially consists.

There are found, both in fresh and salt waters, numerous examples of this Rhizopod type, which do not present any essential advance upon the Amœba and Actinophrys; and a large proportion of these are endowed with a shelly investment which may be either calcareous or siliceous,—the former being the characteristic of the *Foraminifera*, the latter of the *Polycystina*. In some of these testaceous forms, the pseudopodia are put forth only from the mouth of the shell, whilst in other cases this is perforated with minute apertures for their passage; but where there are no such apertures, the sarcode body not unfrequently extends itself over the entire external surface of the shell, and may give off pseudopodia in every direction. Generally speaking, the Foraminifera live attached to sea-weeds, zoophytes, &c.; but their pseudopodia have a very extensive range, and form a sort of animated spider's web, most wonderfully adapted for the prehension of food. The absence of any membranous investment to these threads is clearly indicated by their fusion or coalescence when two or more happen to come into contact; and sometimes a fresh expansion of sarcode takes place at spots remote from the body, so as to form new centres from which a fresh radiation of pseudopodia proceeds.

By far the greater number of Foraminifera are *composite* fabrics, evolved, like zoophytes, by a process of continuous gemmation, each *gemma* or bud remaining in connection with that from which it was put forth; and according to the plan on which this gemmation takes place, will be the configuration of the composite body thereby produced. Where the segments succeed each other in a line, that line is very commonly bent into a spiral; and each new segment being a little larger than the preceding, the spire gradually opens out, so that the shell very closely resembles that of the Nautilus, both in its form and in its chambered structure. There is, however, this essential difference,—that whereas in the nautilus and other chambered shells formed by cephalopod mollusks, the animal lives only in the outermost chamber, all the inner ones having been successively vacated by it, each chamber in the foraminiferous shell continues to be occupied by a segment of the composite body, communicating with the segments within and without by threads of sarcode, which traverse minute passages left in the partitions between the chambers. In the classification of these forms, an extraordinary amount of allowance has to be made for the very wide range of variation that may present itself within the limits of one and the

same specific type. It is very easy to select from any extensive collection of foraminifera, recent or fossil, sets of forms having certain characters in common, but yet so dissimilar in other respects that few naturalists would have any doubt as to their specific or even generic distinctness; yet when the collection is thoroughly examined, such a series of intermediate forms is found to exist, as connects all these by gradations so insensible as to prevent the possibility of any line of demarcation being satisfactorily drawn between them. A remarkable example of this kind is presented by the generic types designated as *Dendritina* and *Peneroplis;* the former being a minute shell, resembling that of the nautilus in its general proportions, and having a single large dendritic aperture in its successive partitions; whilst the latter is flattened, and instead of one large aperture, has a series of small foramina arranged in a single line. Now between these every gradation can be found, both in the form of the shell and in the mode of communication through the septa; the flattened shell of Peneroplis presenting various degrees of turgidity until it attains the proportions of Dendritina; and the linear arrangement of the isolated apertures, in like manner, giving place to one in which they are approximated more closely together into a sort of bundle, still, however, retaining their distinctness; whilst in other individuals, the distinct apertures coalesce into one large jagged orifice, the borders of which become more and more deeply cut, until they present the ramifying extensions characteristic of dendritina. Now if, in such a series, we once begin to make a distinct species for every well marked dissimilarity, either in the form of the shell, or in that of the aperture, we must multiply our species almost indefinitely, contrary to all probability; and there is no medium between doing this, and uniting the whole series of forms included in these two reputed genera under one specific type. This is the more remarkable, because in one locality we may find only the Dendritina-form, in another only the Peneroplis-form, whilst the transitional or intermediate forms come from a third.

Another remarkable example of this wide range of specific characters is presented in the *Orbitolite*, a composite organism, which, originating in a spheroidal nucleus of sarcode, increases by the formation of new segments in concentric rings around this, so that, each segment becoming invested with a shelly envelope, a very beautiful disk is formed, which is enlarged by successive additions to its margin. The segments communicate with each other by annular canals; and there are also passages connecting each annulus with those within and without; whilst from the outermost annulus there are passages opening at the margin of the shelly disk, through which alone the pseudopodia issue that obtain the food for the whole organism. Now there are two very distinct types of growth presented by these Orbitolites; one, namely, in which the disk is very thin, and the segments form (as it were) but a single floor; and the other in which the disk becomes comparatively thick

through the vertical elongation of the segments, which, moreover, are themselves partially divided into at least three distinct stories: two, namely, which form the two surfaces of the disk, and an intermediate one, which is very distinctly separated from them both. The former type of growth may be designated as the *simple*, the latter as the *complex*. Now some Orbitolites seem to go through their whole lives upon the simple plan, whilst in others the complex plan shows itself in the very first ring; and from the comparison of such alone, it might be fairly supposed that these two plans are characteristic of two distinct species. But when a considerable number of these forms are examined, it appears that the simple type may pass into the complex at any period of its growth; the same disk presenting the simple plan in the first 5, 10, 20, 30, or more annuli, and the complex in all those subsequently formed. Hence there can be no question that even so marked a diversity in plan of growth is not in that case sufficient to establish a diversity of specific type, but that the two must be accounted varieties only.

A no less remarkable range of variation has been shown by Prof. Williamson, and Mr. W. K. Parker, to prevail in other groups of Foraminifera which they have particularly studied; so that it would appear as if this type of animal existence were specially characterized by its tendency to such variations. And this will seem the more probable, when it is considered how little of definiteness there is in the form and structure of the sarcode-body that forms the shell; so that the wonder is, not that there should be a wide range of variation both in the form and in plan of growth of the aggregate body, and in the mode of communication of the individual segments, but that there should be any regularity or constancy whatever. But it is only in the *degree* of this range, that this group differs from others; and the main principle which must be taken as the basis of its systematic arrangement,—that of ascertaining the range of specific variation by an extensive comparison of individual forms,—is one which finds its application in every department of natural history, and is now recognised and acted on by all the most eminent zoologists and botanists. There are still too many, however, who are far too ready to establish new species upon variations of the most trivial character, without taking the pains to establish the value of these differences by ascertaining their constancy through an extensive series of individuals,—thus, as was well said by the late Prince of Canino, "describing specimens instead of species," and burdening science not only with a useless nomenclature, but with a mass of false assertions. It should be borne in mind that every one who thus makes a bad species, is really doing a serious detriment to science; whilst every one who proves the identity of species previously accounted distinct, is contributing towards its simplification, and is therefore one of its truest benefactors.

Some of the most interesting physiological and zoological con-

siderations which connect themselves with the study of this group having thus been noticed, its geological importance has in the last place to be alluded to. Traces, more or less abundant, of the existence of Foraminifera are to be found in calcareous rocks of nearly all geological periods; but it is towards the end of the secondary, and at the beginning of the tertiary period, that the development of this group seems to have attained its maximum. Although there can be no reasonable doubt that the formation of Chalk is partly due to the disintegration of corals and larger shells, yet it cannot be questioned that in many localities a very large proportion of its mass has been formed by the slow accumulation of foraminiferous shells, sometimes preserved entire, sometimes fragmentary, and sometimes almost entirely disintegrated. The most extraordinary manifestation of this type of life, however, presents itself in the "nummulitic limestone," which may be traced from the region of the Pyrenees, through that of the Alps and Apennines, into Asia Minor, and again through Northern Africa and Egypt, into Arabia, Persia, and Northern India, and thence (it is believed) through Thibet and China, to the Pacific, covering very extensive areas, and attaining a thickness in some places of many thousand feet; another extensive tract of this nummulitic limestone is found in the United States. A similar formation, of less extent but of great importance, occurs in the Paris basin; and it is not a little remarkable that the fine-grained and easily-worked limestone, which affords such an excellent material for the decorated buildings of the French metropolis, is entirely formed of an accumulation of minute foraminiferous shells. Even in the nummulitic limestone, the matrix in which the nummulites are imbedded, is itself composed of minute Foraminifera, and of the comminuted fragments of larger ones. The remarkable discovery has been recently made by Prof. Ehrenberg, that the green and ferruginous sands which present themselves in various stratified deposits, from the Silurian to the Tertiary epoch, but which are especially abundant in the Cretaceous period, are chiefly composed of *casts* of the interior of minute shells of Foraminifera and Mollusca, the shells themselves having entirely disappeared. The material of these casts, which is chiefly silex, coloured by silicate of iron, has not merely filled the chambers and their communicating passages, but has also penetrated, even to its minutest ramifications, that system of interseptal canals, whose existence, first discovered by Dr. C. in Nummulites, has been detected also in many recent Foraminifera allied to these in general plan of structure. And it is a very interesting pendant to this discovery, that a like process has been shown by Prof. Bailey, to be at present going on over various parts of the sea bottom of the Gulf of Mexico and the Gulf Stream; casts of Foraminifera in green sand being brought up in soundings with living specimens of the same types.

[W. B. C.]

WEEKLY EVENING MEETING,

Friday, March 19.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Henry Thomas Buckle, Esq.

On the Influence of Women on the Progress of Knowledge.

The two leading propositions which Mr. Buckle attempted to establish, are, 1st,—That women are naturally deductive, men inductive: 2nd,—That women, by encouraging and keeping alive habits of deductive thought, have unconsciously accelerated the progress of knowledge by preventing men from being as exclusively inductive, or, in other words, as one-sided as they would otherwise be.

The influence thus exercised by women escapes general attention, because it displays itself not in making discoveries, but in affecting the method according to which discoveries are made. Knowledge is commonly divided only into two parts, art and science; but it does in reality contain a third department which is superior to the other two, and which it is the business of philosophy properly so called, to investigate. This third, or highest department, is method; and in studying any subject, the first question should always be, "What is the proper method of proceeding?" There are only two methods, the inductive and the deductive. Induction proceeds from the external world to the internal, *i.e.*, from facts which are presented to the senses, to ideas which are presented to the mind. Deduction proceeds from the internal world to the external, *i.e.*, from ideas to facts. Women are less practical than men; they are more enthusiastic, more emotional, and live in a more ideal world. They, therefore, naturally prefer a method which proceeds from ideas to facts, leaving to men the other method of proceeding from facts to ideas. These two methods, though often united, are essentially distinct; and even supposing that all ideas are suggested by the external world (an assertion which is constantly made, but has never been proved), this would merely affect the question as to the *origin* of the *elements* of our knowledge, and would leave untouched the *method* by which those elements are subsequently arranged. If, for example, as many affirm, the axioms of geometry are the result of induction, it is nevertheless certain that the induction takes place at so early a

period of life, that we are unconscious of it. Hence we are justified in terming geometry a deductive science, even if we admit that its origin is inductive; because the great labour consists in the subsequent process of working deductively from ideas, which (according to this view) we have inductively obtained.

In England it is constantly stated that since Bacon all great physical discoveries have been made by induction, in opposition to the more ideal method. If this be true, the deductive influence of women must, in reference to such discoveries, have done more harm than good. But Mr. Buckle asserted that it is not true; and he corroborated his assertion by an analysis of the method adopted by Newton, Haüy, and Göthe, in regard to their discoveries of the law of Gravitation, the law of the Derivation of the Secondary Forms of Crystals, the Morphological Law of Vegetables, and the law of the Vertebral Composition of the Cranium.

Finally, Mr. Buckle observed, that an exclusive employment of the inductive philosophy was contracting the minds of physical inquirers; gradually shutting out speculations respecting causes and entities; limiting the student to questions of distribution, and forbidding to him questions of origin; making everything hang on two sets of laws, namely those of co-existence and of sequence; and declaring beforehand how far future knowledge can carry us. But we shall not always be satisfied with seeing the laws of nature rest on this empirical basis; and the most advanced thinkers are looking to a period when we shall deal with problems of a much higher kind than any yet solved; when we shall incorporate mind and matter into a single study; when we shall seek to raise the veil and penetrate into the secrets of things. Everything indicates that a struggle of this sort is impending, and to achieve success the imagination will have to aid the understanding more than it has yet done. We shall need every faculty, every resource, and every method. The intellectual peculiarities of both sexes must be combined, before we can expect to conduct to a prosperous issue that great contest between Man and Nature, of which this generation may witness the beginning, but of which our distant posterity can hardly hope to see the end.*

[H. T. B.]

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* Mr. Buckle's Discourse is given in full in "Fraser's Magazine," for April 1858.

WEEKLY EVENING MEETING,

Friday, March 26.

Sir Benjamin Collins Brodie, Bart. D.C.L. F.R.S. Vice-President, in the Chair.

Rev. John Barlow, M.A. F.R.S. Vice-Pres. & Sec. R.I.

On Mineral Candles and other Products manufactured at Belmont and Sherwood.

The candles and the other products (liquid hydro-carbons), on which Mr. Barlow discoursed, are manufactured by Price's Candle Company, at Belmont and Sherwood, according to processes patented by Mr. Warren De la Rue. The novelty of these substances consists—1. In the material from which they are obtained. 2. In the method by which they are elaborated. 3. In their chemical constitution.

1. The *raw material* is a semifluid naphtha, drawn up from wells sunk in the neighbourhood of the river Irrawaddy, in the Burmese empire. The geological characteristics of the locality are sandstone and blue clay. In its raw condition the substance is used by the natives as a lamp-fuel, as a preservative of timber against insects, and as a medicine. Being in part volatile, at common temperatures, this naphtha is imported in hermetically-closed metallic tanks, to prevent the loss of any constituent. Reichenbach, Christison, Gregory, Reece,* Young,† Wiesman (of Bonn), and others have obtained from peat, coal, and other organic minerals, solids and liquids bearing some physical resemblance to those procured from the Burmese naphtha; but the first-named products have, in every instance, been formed by the decomposition of the raw material. The process of De la Rue, is, from first to last, a simple separation, without chemical change.

2. *The processes adopted.*—In the commercial processes, as carried out by Mr. George Wilson, at the Sherwood and Belmont Works, the crude naphtha is first distilled with steam at a temperature of 212° Fahr.; about one-fourth is separated by this operation. The distillate consists of a mixture of many volatile hydro-carbons; and it is extremely difficult to separate them from each other on account of their vapours being mutually very diffusable, however different may be their boiling points. In practice,

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* See Proceedings of the Royal Institution, Vol. I., p. 4. † Ibid, p. 135.

recourse is had to a second or third distillation, the products of which are classified according to their boiling points or their specific gravities, which range from ·627 to ·860, the lightest coming over first. It is worthy of notice, that though all these volatile liquids were distilled from the original material with steam of the temperature of boiling water, their boiling points range from 80° Fahr. to upwards of 400° Fahr.

These liquids are all colourless, and do not solidify at any temperature, however low, to which they have been exposed.* They are useful for many purposes. All are solvents of caoutchouc. The vapour of the more volatile, Dr. Snow has found to be highly anæsthetic. Those of the lower specific gravity, called in commerce *Sherwoodole*, have great detergent power, readily removing oily stains from silk, without impairing even delicate colours. The distillates of higher specific gravity are proposed to be used as lamp-fuel; they burn with a brilliant white flame; and, as they cannot be ignited without a wick, even when heated to the temperature of boiling water, they are safe for domestic use.

A small per-centage of hydro-carbons, of the benzole series, comes over with the distillates in this first operation. Messrs. De la Rue and Müller have shown that it may be advantageously eliminated by nitric acid. The resulting substances, nitro-benzole, &c., are commercially valuable in perfumery, &c.

After steam of 212° has been used in the distillation just described, there is left a residue, amounting to about three-fourths of the original material. It is fused, and purified from extraneous ingredients (which Warren De la Rue and H. Müller have found to consist partly of the colophene series) by sulphuric acid. The foreign substances are thus thrown down as a black precipitate, from which the supernatant liquor is decanted. The black precipitate, when freed from acid by copious washing, has all the characteristic properties of native asphaltum. The fluid is then transferred to a still, and, by means of a current of steam made to pass through heated iron tubes, is distilled at any required temperature. The distillates obtained by this process are classed according to their distilling-points, ranging from 300° to 600° Fahr. The distillations obtained, at 430° Fahr. and upwards, contain a solid substance, resembling in colour and in many physical and chemical properties, the paraffine of Reichenbach; like it it is electric, and its chemical affinity is very feeble: but there are reasons for believing that a difference exists in the atomic constitution of the two substances. The commercial name of *Belmontine* is proposed for the solid derived from the Burmese naphtha. Candles manufactured from this material possess great illuminating power. It is stated that a Belmontine candle, weighing $\frac{1}{8}$th lb., will give as

* The freezing mixture of solid carbonic acid and ether does not affect the fluidity of these bodies.

much light as a candle weighing ⅛th lb., made of spermaceti or of stearic acid. Its property of fusing at a very low temperature into a transparent liquid, and not decomposing below 600° Fahr. recommends this substance as the material of a bath for chemical purposes. As to the fluids obtained in the second distillation, already described, they all possess great lubricating properties; and, unlike the common fixed oils, not being decomposable into an acid, they do not corrode the metals, especially the alloys of copper, which are used as bearings of machinery. This aversion to chemical combination, which characterizes all these substances, affords, not only a security against the brass-work of lamps being injured by the hydro-carbon burnt in them, but also renders these hydro-carbons the best detergents of common oil lamps. It is an interesting physical fact, that some of the non-volatile liquid hydro-carbons possess the fluorescent property which Stokes has found to reside in certain vegetable infusions.

3. *Chemical constitution of these hydro-carbons.*—On this subject, there will be found a short memoir by Warren de la Rue, and Hugo Müller, in the Proceedings of the Royal Society, Vol. viii., page 221. The researches referred to in that memoir are nearly completed. The principal constituents of the Burmese naphtha, are—(*a*), (the largest in proportion) a substance identical in composition with either the hydrurets or the radicals of the ethyle series; (*b*) Substances of the benzole series, forming a comparatively small portion. It has, however, been ascertained that some of the hydro-carbons of this aromatic series differ in their chemical and physical properties from the analogous members of the same series obtained from the usual sources. This difference is most strongly marked in the case of cumole and its higher homologues of the benzole series,* (*c*) the colophene series already adverted to.

An important characteristic of the Burmese naphtha is its being almost entirely destitute of the hydro-carbons belonging to the olefiant-gas series.

[J. B.]

* In illustration of this view may be cited, Church's discovery of a parabenzole in coal tar, boiling at 185° Fahr., and not solidifying at 32°.

Royal Institution of Great Britain.

GENERAL MONTHLY MEETING,

Monday, April 5, 1858.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

The Marchioness of Londonderry.
Henry Diedrich Jencken, Esq.
William Jones Loyd, Esq.
Sir Thomas Phillips, and
Major-General Westropp Watkins

were duly *elected* Members of the Royal Institution.

Herbert Barnard, Esq.
Thomas Hyde Hills, Esq.
John Pyle, Esq. F.R.C.S. and
William Southey, jun. Esq.

were *admitted* Members of the Royal Institution.

The following Presents were announced, and the thanks of the Members returned for the same :

From

Her Majesty's Government—Mortality of the British Army, as compared with the Mortality of the Civil Population in England. fol. 1858.

The Government of British Guiana — Monthly Tables of Meteorological Elements deduced from Observations at Demerara, from 1846 to 1856. By P. Sandeman. 4to. 1857.

Hon. East India Company—Geological Papers on Western India: with an Atlas. Edited by H. J. Carter. 8vo. and 4to. Bombay, 1857.

Agricultural Society of England, Royal—Journal, Vol. XVIII. Part 2. 8vo. 1857.

Astronomical Society, Royal—Monthly Notice, March 1858. 8vo.

Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for March 1858. 8vo.

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WEEKLY EVENING MEETING,

Friday, April 16.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President, in the Chair.

ROBERT GODWIN-AUSTEN, Esq. F.R.S. & G.S.

On the Conditions which determine the probability of Coal beneath the South-Eastern parts of England.

FOSSIL fuel may be of any geological age: seams and traces of it have often encouraged researches amongst the tertiary strata of the London basin: even within the last few years there has been a Woking Heath coal-mining adventure; and it has long been a matter of popular belief that Blackheath is to supply London with coal. There are, however, thick accumulations of tertiary fuel, of which Bovey, in South Devon, is the best example in this country. There is coal belonging to the period of the chalk—there have been innumerable trials for coal amongst the fresh-water formations of the Weald of Kent, Surrey, and Sussex—there is tolerable coal associated with the oolitic series of Yorkshire; but the coal to which the following speculations refer is that which is derived from what have been designated the "true coal measures."

The period to which this coal belongs in the earth's history is of very great antiquity; but the usual way of representing its age by reference to a vertical scale of geological formations, is inapplicable in the present case; and the only way in which it can be stated is this, that the whole series of formations which may be seen in the cliffs of the south coast of England, from Torbay to the Isle of Wight, have been accumulated since the period of the "true coal" series.

The superficial extent of the carboniferous series in this country is very great: allowing for what has been denuded, and what we know is covered up, it may be described as extending in a broad band from Berwick diagonally across the whole island into South Wales, and thence across the county of Devon.

The usual subdivision of the great carboniferous series into a descending series of "coal measures," "mountain limestone," and red sandstone, is geographical; the area of sandstone with coal plants is western; that of the limestone is central; and true coal measures occur unconnected with either. The carboniferous form-

ation, as a whole, exceeds that of any other geological group in this country, considered with reference to surface.

What is coal? It is pure vegetable matter—the product of plant-growths. And with respect to the mode by which it has been accumulated, two theories have been proposed: there is the "drift" theory, which accounts for its occurrence as the accumulations of vegetable matter, brought down by mighty rivers, and deposited in lakes and sea-margins. There is something too turbulent in this theory to account for our great seams of fossil fuel.

The other theory is, that coal is the product of a vegetation which grew upon the very spots, and covered the areas over which our coal-beds extend, like the peat-beds of the present day. This is the theory of M. De Luc, M. Ad. Brongniart, and Messrs. Lindley and Hutton.

In supposing that coal originated as peat, all that is meant is, that it is the product of a vegetation composed of like plants, such as could live on in association over the same spots, growing above and decaying beneath; but differing as widely in the plants which composed it from our present peat plants, as did the whole of the vegetation of that period from that of the present period: the huge stigmariæ are wholly unlike any plants which commence the peat growth now.

The succession of a coal-field may be seen in a small scale in the deposits of lakes which have had differences of level from local accidents; and with reference to extent Ireland may be taken as an illustration of continuous masses of vegetable matter, of vast thickness, covering the whole country for 50 miles, and at low levels. Depress Ireland ever so little, so that the waters of the sea should reach in in some places, and the river waters, such as those of the Shannon, should collect into lakes; and just in proportion as the water was shallow would an uniform stratum of sand, or silt, or gravel, be spread out above the peat-growths.

The history of the coal-fields of this and every country, is that of an endless succession of such changes.

The question of the probable existence of coal measures at any given spot over the European area depends primarily on the original form of the surface of these coal growths: in other words, can we construct a map of Western Europe for the coal measure period?

The restoration of the physical features of a portion of the earth, for any given past period, is not so difficult, nor so purely speculative as some may imagine: every form and combination of mineral materials composing the sedimentary formations, all the forms of life they contain, serve to indicate the precise conditions under which they have been accumulated. Shingle and gravel mark marginal zones, sand zones mark lower or submarginal regions, deep sea deposits consist of mud or oaze; thousands of persons who have never even heard of the inquiries of the geolo-

gist, have doubtless argued that Blackheath, with its rounded shingle, must at some time or other have been at the sea-side. Assemblages of marine shells are the evidences of former seas; land and fresh-water shells and plants of old lakes and terrestrial conditions.

By the aid of such guides as these, the form of the area of the coal measures may be defined. Commencing in the west, we have early indications of the proximity of dry land and fresh-water accumulations. The earliest carboniferous deposits contain fern-like plants in wonderful profusion and beauty: with them are "pond muscles" (*anodon*). The land here lay to the south. The depositions of the North of Ireland require the existence of a wide expanse of dry land somewhere beyond it on the north. The Wicklow mountains were part of the dry land of the coal period. In the beds of the carboniferous limestone near Dublin may be seen angular fragments of the peculiar granite of these mountains, and which must have been floated away by seaweeds from a shore line, just as happens now. Dry land connected the Wicklow mountains with those of Wales. If we pass over this interval we find evidence that the mountains of Wales were then dry land. The conditions of portions of the coal measures bordering on this region have been investigated by most competent geologists, Sir R. Murchison and Mr. Prestwich. In the Shrewsbury district are pure fresh-water limestones. Coalbrook-dale, throughout the whole accumulation of its beds, seems to have been immediately subordinate to an area of dry land. The great Yorkshire coal series, which has been so well described by Professor Phillips, is wholly lacustrine, with the exception of one intercalated band of marine limestone.

The proximity of dry land to the Edinburgh coal-field has been shown by the researches of Dr. Hilbert and Mr. L. Horner, in the fresh-water deposits of Burdie. The mountains of Cumberland were dry land, and so all those of the border counties which range from Wigtonshire to Berwick. All the mountains of the western highlands of Scotland, an area extending north beyond the Shetlands, and westwards into the Atlantic, was also land surface: a vast tract lay in this (the north) direction, of which the great Scandinavian chain alone remains, and which supported the rivers which bore down the waste of granitic and crystalline rocks which enter so largely into the coal-measure sandstones of our northern districts.

Passing across into the Cotentin, we find a series of coal formation, skirting the old mountain ranges of the north-west of France.

The great central granitic plateau of France is fringed with coal growths, and over the whole of its surface, are innumerable small coal-fields, the lacustrine accumulations of the valleys of that region—this was an upland coal region.

The Vosges mountains have been raised over a surface which was dry land, and was connected with the Schwartzwald, the

Odenwald, and the Spessart, and a great tract extending north and east, whence came down that curious assemblage of terrestrial forms which has been met with in the great fluviatile and lacustrine deposits of the Saarbruch coal basin. Such is the form of the area which contains the great coal formations of western Europe.

The island which is represented in the interior of that great basin is not imaginary*—evidence of direction and extent of southern coast line from shingle bed of Burnot. The extension of a band of shingle from beyond Eupen to the Boulonnais, marks the direction of an old coast line which lay to the north of it. It was from this mass of land that the terrestrial vegetation, and the fresh-water shells so abundant in the Liege coal measures, were derived.

The whole area, as here described, may be compared, as to its physical characters, with large level tracts which lie west of the Blue Mountains in Australia, into which the Lachlan, the Darling, the Murrumbidgee, and the Murray discharge.

Between the close of the coal growths, and the period of the formation which next succeeded, the surface of the whole of the area which has been sketched out was disturbed and broken up. Some of the lines, like that of our Pennine Chain, conform to those masses of terrestrial surface which tended in that direction; *and a very remarkable line is one which has a general east and west direction across the European area.* This line also conforms to the direction of old land which was to the north and south of it, and comprises the whole of the interval between the coal growth surfaces of the Saarbruch districts, and those of Belgium.

The Section along the Meuse affords good illustrations of the character of this band of disturbed strata; in this section the upper beds of the coal measures occupy the deep troughs; the older parts of the Palæozoic series appear in the ridges. Such is the character of the great Liege, Namur, Mons, Valenciennes coal band throughout.

The line which passes along the south of this coal band, was a *boundary line for the oolitic formations, and for the earliest accumulations of the cretaceous period,*—this is particularly well seen in the Boulonnais.

The question as to the probability of coal in this (south-east) part of England, depends on the relation between the physical configuration of the present surface as compared with this older surface.

The character of the axis or ridge of Artois, with its valleys of elevation, was described as a continuation of the line of disturbance along the south of the Mons coal band, in the east, and as coinciding with the north escarpment of the Boulonnais on the west. The Boulonnais is physically a portion of the great elliptical denudation of Weald, of which the North Downs from Dover, west, are

* The reference here made is to a map which represented the physical features of Western Europe, at the period of the coal growth.

a continuation of the chalk range from Wissant, east. This line of disturbance is continued on by the valleys of elevation of Highclere, King's-clere, &c., and opens out into the valley of Devizes, forming a great linear anticlinal ridge, which coincides with the axis of old red sandstone of Frome, supporting the coal-fields of Somerset on the north.

The principle on which the existence of a band of coal measures may be conjecturally placed along the south-east counties of England, is this,—that like physical features have a like significance —the precise probability of the continuity of the coal-band along our south-east area is great, and every fresh point of agreement adds strength to that probability; so that when these amount to three or four, the evidence may be deemed conclusive.

The Kentish-Town artesian well passed through the white chalk and gault, a shingle brand of old sedimentary and crystalline rocks, ending on micaceous sandstones, at a high angle. Here the points of agreement with the French and Belgic sections, were, 1st, the absence of the oolitic series; 2nd, of the lower cretaceous strata; and 3rd, the occurrence of the tourtia or shingle brand, as in Flanders and the north of France.

The artesian well at Harwich found the chalk resting in old clay slate, with cleavage structure, and micaceous sandstones; and from the presence of a Posidonia, may be referred to the culm series of the Rhenish provinces or of Devonshire; in this instance there is a perfect agreement with the condition of surfaces which extend north from the Belgian coal-band.

By the help of these points, we can trace the arrangement of the old rocks beneath our south-east counties. The limiting boundary of the oolitic series, and of the lower green sand, lies south of London. The coal-trough conforms to the valley of the Thames and Kennet; older rocks still, such as those of the Belgian series, rise to the north; beyond which, at the distance of Harwich, the coal series is again brought in.

The existence of coal beneath Blackheath is therefore not so great an improbability as was once supposed; nor in the absence of the whole series of secondary formations, from the white chalk downwards, is its depth probably very great.

[R. G.-A.]

WEEKLY EVENING MEETING,

Friday, April 23.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Col. Henry James, R.E. F.R.S.

On the Geodetic Operations of the Ordnance Survey.

The Geodetic operations of the survey include the triangulation and levelling, which extends over the whole United Kingdom, the measurement of arcs of meridians, and the determination of the figure, dimensions, and mean specific gravity of the earth.

The special object for which these operations are required was first described, and then, the methods employed in performing them were briefly sketched.

The triangulation, consists, 1st of a *primary* series of triangles, the sides of which are some of them upwards of 100 miles in length, and the stations of which are placed on the highest mountains; such for example, as Snowdon, in Wales, Sca Fell, in Cumberland, and Slieve Donard, in the county of Down, in Ireland; the sides of this triangle are each upwards of 100 miles long. This great triangulation extends over the United Kingdom;—a series of stations are then selected to form a *secondary* triangulation, the sides of which are from 10 to 15 miles long; and again another series of stations are selected to form the *minor* or *tertiary* triangulation, the sides of which are about three-quarters of a mile in length; and thus the whole country is covered with a connected network of triangles, to form the basis of the detailed survey which is now in progress. Upon the accuracy with which this portion of the work is executed, mainly depends the character of the national survey for accuracy in its most important features.

The minor triangulation is that which is immediately used for the detailed survey: the field surveyors actually measure the length of each side with their chains, and cross lines are also measured within the triangles. As the length of each side is previously known, the correctness with which the surveyors perform their work is tested in the office, and accuracy insured in every part. The establishment of a general triangulation also enables the engineer to employ large numbers of surveyors at the same time, and without any fear of the work being distorted in any direction, as every

object on the ground must, under this arrangement, be accurately represented in its true relative position to all others, however distant they may be. Thus the houses represented on a plan of a parish in the centre of the kingdom are not only in their correct relative position to the houses in their neighbourhood, but also to every other house, whether in Caithness or Cornwall.

The levels on the plans are all given with reference to the mean tide level at Liverpool, or the line above and below which the tide rises and falls. Lines of levels from this datum have been carried through all parts of the country; and thus the levels also are in correct relation to each other, however distant the points may be which are compared.

The curious circumstance was adverted to, that the levelling taken in Ireland, connecting the mean tide level at a series of stations all round the coast, seemed to establish the fact, that the plane of the mean tide level was inclined from N.W. to S.E., and was three feet higher on the coast of Donegal than on the coast of Wexford.

The speaker was not able to offer any other possible explanation for this, than that of the impinging of the warm water of the Gulf Stream upon the north-west coast of Ireland, and he offered this as a mere conjecture.

A set of the Ordnance Plans as now produced were exhibited at the meeting. They consist of

1. Plans of *Towns*, on the $\frac{1}{500}$th scale, or 42 feet to an inch.
2. Plans of *Parishes*, on the $\frac{1}{2500}$th scale, or 25· inches to a mile; or one square inch to one acre.
3. Plans of *Counties*, on the scale of 6 inches to a mile.
4. Map of the *Kingdom*, on the scale of 1 inch to a mile.

All the plans which are drawn on the larger scales, are reduced by photography to the smaller, and at a very trifling cost, and, as compared with all former methods of reduction, with marvellous rapidity. This method of accurately reducing plans was first introduced by Col. James, and will effect a very great saving in the cost of the survey. The plans of the parishes are zincographed, but all the others are engraved on copper.

The methods employed for conducting the geodetic operations were then described.

The first consideration is the obtaining an accurate standard of length;—the Ordance standard of length is a bar of iron, on which the length of 10 feet as derived from the old Parliamentary standard yard was set off. But this standard yard having been destroyed in the fire which consumed the Houses of Parliament, a new standard has been constructed, by a commission, of which the Astronomer-Royal, and Mr. Sheepshanks and Mr. Baily were members. The superintendent of the survey had therefore to ascertain the length

of the Ordnance standard in terms of the new standard of length; and for this purpose he had an intermediate standard constructed on which $3\frac{1}{3}$ lengths of the standard yard were set off, and the 10 feet thus derived compared with the original 10 foot Ordnance standard. From the comparisons made between these, we have a proof of the accuracy with which the national standard of length has been restored; the difference would not, in fact, amount to more than the $\frac{1}{13}$th of an inch in a mile.

With the 10 foot standard for reference, the late General Colby, who for many years so ably superintended the survey, designed his *Compensation Bars*, for the measurement of the base lines for the triangulation. General Colby, availing himself of the known unequal expansion of brass and iron, combined bars of these metals in the base-measuring apparatus, in such a way as to preserve the distance between two points on the tongues connecting the bars, at the constant distance of 10 feet, under every change of temperature.

In the actual measurement of the bases, these bars were ranged in a perfectly straight and horizontal line, and to prevent any possible disturbance in the position of the first laid bars, they were separated by an interval of six inches, the interval itself being measured with a double microscope, the foci of the microscopes being exactly at six inches apart, and their invariability secured by the bars connecting the two, being made to compensate each other's expansion, in the same way that the 10 feet bars are compensated. A central microscope between the two described serves as a pivot for reversing them, and also for the purpose of establishing fixed points on the ground, as points of reference in the remeasurements taken.

Sir John Herschel and Mr. Babbage were present when 500 feet of the base at Lough Foyle were remeasured, and the error amounted to only a third (by estimation) of the breadth of the very finest dot which could be made with the point of a needle. We have thus an assurance of the extreme accuracy with which the two bases, one on Salisbury Plain, the other on the shores of Lough Foyle, in the north of Ireland, each about seven miles long, were measured. These base lines may, in fact, be described as air-lines drawn from the fine dot at one extremity to that at the other.

Having established an accurate base, the next operation is to establish some trigonometrical stations to form triangles with it; and then, by means of a theodolite, the centre of which is accurately adjusted over the dots at the extremities of the base, measuring the angles between the stations, the data are obtained for computing the length of the sides of the triangles. The sides of the triangles thus obtained become new bases, from which the length of the sides of other triangles are in like manner computed; and in this way the exact length of every line in the great network of the triangulation is accurately known. Of the accuracy with which the angles were taken, we have, first, the proof by the summation of the angles in

each triangle: and, secondly, the proof arising from a comparison of the computed length of one base, as derived from the angular measurements, and the actual length from the linear measurements.

The difference between the computed length of the Lough Foyle base, through the triangles extending from it to Salisbury Plain, a distance of 360 miles, and the actual measured length, was five inches.

But as this error could not be attributed to one base rather than the other, a *mean base* was established by a correction to each in the proportion of the square roots of their lengths, so that computing from the mean base, the measured bases have apparent differences of $+$ or $-$ $2\frac{1}{2}$ inches.

Three other bases were measured with Ramsden's 100 feet steel chains, one on Hounslow Heath, another at Misterton Car, near Doncaster, and the third at Belhelvie, near Aberdeen; and the measured lengths of these bases, differed in no instance 3 inches from the lengths as computed from the mean base.

The observed angles have also been so corrected as to render the triangulation consistent in every part; and the result is, that taking any side of any triangle as a base, and computing in any way through the triangulation, the same length will be reproduced. The triangulation may, therefore, be said to be perfect in every respect.

The latitudes of 32 of the principal stations were observed with Ramsden's great zenith sector, which was afterwards burnt in the great fire at the Tower, and with Airy's zenith sector, which was made expressly for the survey.

If the figure of the earth be first supposed to be a sphere, it is obvious that the length of every degree of latitude would be equal, and that when the length of a certain number of degrees of latitude is accurately known, we have all that is required to compute the length of the 360 degrees of a great circle of the earth, and of the length of its diameter; but if the figure of the earth is not a sphere, but a spheroid compressed at the poles, then the length of each degree, as measured towards the poles, will be unequal and continually increasing, and this is found from observation to be the actual fact. Thus, for example, the length of a degree in the parallel of Edinburgh is 100 yards longer than a degree at Southampton; and in the Shetland Islands, it is 200 yards longer; and from a knowledge of the length of the several portions of arcs of meridians measured in this and other countries, the true figure and dimensions of the earth are known.

The elements of the spheroid which most nearly represent all the distances and latitudes, are

$$\begin{array}{lcl} \text{Polar diameter} & = & 7899{\cdot}5 \text{ miles.} \\ \text{Equatorial} \quad . & = & 7926{\cdot}5 \text{ miles.} \\ \text{Ellipticity} \ . \quad . & = & \dfrac{1}{294} \end{array}$$

The elements of the spheroid, given in Airy's "Figure of the Earth," are

$$\begin{aligned} \text{Polar diameter} &= 7899 \cdot 1 \text{ miles.} \\ \text{Equatorial} \quad . &= 7925 \cdot 6 \text{ miles.} \\ \text{Ellipticity} \ . \quad . &= \frac{1}{299 \cdot 3} \end{aligned}$$

Our most recent determination, therefore, slightly increases the ellipticity, and we increase the equatorial diameter of the earth by about one mile.

One of the chief difficulties which is encountered in the investigation of the figure of the earth, arises from the local attraction at the stations at which the observations for latitude are taken, in consequence of the irregular distribution of the masses of matter in the mountains or hills near the stations, or the unequal density of the matter beneath the surface of the earth.

Thus, for example, when observations are taken on the north end of the hill at Dunnose, in the Isle of Wight, and also at the south end, the great mass of the hill being between the two stations, the difference of latitude is found to be greater than is due to the actual distance between the stations; and this, because the attraction of the mass of the hill has drawn the plumb line in each case towards it, and made the celestial arc greater than the geodetic.

The detailed survey of Edinburghshire having been published with the contours, or zones of equal altitude, engraved on the plans, and thus furnishing accurate information as to the relief of the ground, the superintendent of the survey undertook, in 1854, to investigate the amount of the local attraction at Arthur's Seat; and this the more readily, as it would furnish the data for computing the mean density of the earth itself. Observations for latitude were taken at the north and south ends of the mountain, and also on the summit, and the geological structure and specific gravity of the rocks composing it ascertained.

The attraction of the mountain was computed, by supposing it divided into a number of vertical prisms, and summing their separate attraction, resolved into the direction of the meridian.

The attraction of each prism is, according to the known laws of gravitation, proportioned to the mass, and inversely proportioned to the square of its distance; similarly, the attraction of the earth is in proportion to its mass, and inversely as the square of the distance from the centre, the ratio of these attractions is equal to the tangent of the angle of deflection. This will be obvious from the inspection of a diagram, on which the attraction of the earth is represented by a vertical line, and the attraction of the mountain by a short line drawn at right angles to it, showing the extent to which the plumb line is deflected or drawn towards the mountain.

Then, if the mountain be assumed to be of the same specific gravity as the earth, the computed deflection at the

South end	. . .	$= 4''\cdot 2$
North end	. . .	$= 3\cdot 8$
Or the whole disturbance		$= 8\cdot 0$

but the observed sum of the deflections, that is, the excess of the celestial arc above the geodetic arc, was found to be only 4·07, or little more than ½ what it would have been had the earth and mountain been of the same specific gravity; and consequently the earth must be of nearly double the specific gravity of the mountain.

The specific gravity of the mountain was ascertained to be 2·75, and therefore as 4·07 : 8·0 : : 2·75 = 5·45 the mean density of the earth; by employing the full number of decimals, we have 5·316 as the mean density of the earth. From similar observations at Schehallien Mountain, Hutton derived the mean density = 5·0. From experiments on the attraction of balls,

Cavendish obtained	. .	5·44
Baily ,,	. .	5·67
Reich ,,	. .	5·44

The Astronomer-Royal, from experiments with pendulums on the surface of the earth, and at a great depth, obtained 6·55.

Col. James concluded his address by saying, "I have endeavoured to give what may be called a mere outline sketch of the geodetic operations of the survey. A full account of all these operations, and of the very intricate and laborious computations which have been made, has just been published. This account has been drawn up by Capt. Alexander Clark, R.E., who is employed with me on the survey, and I must refer all those who desire to have more precise information on these subjects to it.

"But I trust it will be understood, from what I have said, how necessary and important these operations are for the execution of a survey with that perfect accuracy which the nation has a right to expect from the officers entrusted with its execution; and that we have, at the same time, contributed data for determining the exact figure, dimensions, and specific gravity of the earth, which form the only units of measure for estimating the distances, the size, and the specific gravity of all the heavenly bodies which surround us."

[H. J.]

[The standard of length, and the compensation-bars used in the measurement of the bases, were exhibited in the lecture-room and described, and a series of diagrams were referred to in the course of the lecture.]

WEEKLY EVENING MEETING,

Friday, April 30.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Professor Andrew C. Ramsay, F.R.S.

On the Geological Causes that have influenced the Scenery of Canada and the North-Eastern Provinces of the United States.

It is impossible thoroughly to explain all the points of this discourse without the aid of the pictorial illustrations and sections employed on the occasion, and therefore in this abstract only some of the leading geological features are noticed.

The island of Belleisle and the Laurentine chain of mountains between the shores of Labrador and Lake Superior consist of gneissic rocks older than the Huronian formation of Sir Wm. Logan. This gneiss is probably the equivalent of the oldest gneiss of the Scandinavian chain, and of the north-west of Scotland, underlying that conglomerate, which, according to Sir Roderick Murchison, in Scotland represents the Cambrian strata of the Longmynd and of Wales. The mountains of the Laurentine chain present those rounded contours that evince great glacial abrasion; and among the forests north of the Ottawa the mammillated surfaces were observed by the speaker to be often grooved and striated, the striations running from north to south. The whole country has been moulded by ice. Above the metamorphic rocks, in the plains of Canada and the United States south of the St. Lawrence, and around Lake Ontario and Lake Erie, the Silurian and Devonian strata lie nearly horizontally, but slightly inclined to the south. Consisting of alternations of limestone and softer strata, the rocks have been worn by denudation into a succession of terraces, the chief of these forming a great escarpment, part of which, by the river Niagara, overlooks Queenston and Lewiston, and capped by the Niagara limestone, extends from the neighbourhood of the Hudson to Lake Huron. Divided by this escarpment the plains of Canada bordering the lakes, and part of the United States, thus consist of two great plateaux, in the lower of which lies Lake Ontario, Lake Erie lying in a slight depression in the upper plain or table land, 329 feet above Lake Ontario. The lower plain consists mostly of Lower Silurian rocks, bounded on the north by the metamorphic hills of the Laurentine

chain. The upper plain is chiefly formed of Upper Silurian and Devonian strata. East of the Hudson, the Lower Silurian rocks that form the lower plain of Canada become gradually much disturbed and metamorphosed, and at length rising into bold hills trending north and south, form in the Green Mountains part of the chain that stretches from the southern extremity of the Appalachian Mountains to Gaspé, on the Gulf of St. Lawrence. Between the plains of the lakes and this range, the steep terraced mass of the Catskills, formed of old red sandstone, lies above the Devonian rocks facing east and north in a grand escarpment.

The whole of America south of the lakes, as far as latitude 40°, is covered with glacial drift, consisting of sand, gravel, and clay, with boulders, many of which, during the submergence of the country, have been transported by ice several hundred miles from the Laurentine chain. Many of these are striated and scratched in a manner familiar to those conversant with glacial phenomena. When stripped of drift all the underlying rocks are evidently ice-smoothed and striated, the striations generally running more or less from north to south, indicating the direction of the ice-drift during the submergence of the country at the glacial period. The banks of the St. Lawrence, near Brockville, and all the Thousand Islands, have been rounded and *moutonnée* by glacial abrasion during the drift period.

The submergence of the country was gradual, and the depth it attained is partly indicated in the east flank of the Catskill mountains. This range, near Catskill, runs north and south, about 10 or 12 miles from the right bank of the Hudson. The undulating ground between the river and the mountains is seen to be covered with striations wherever the drift has been removed. These have a north and south direction; and ascending the mountains to Mountain House, the speaker observed that their flanks are marked by frequent grooves and glacial scratches, running not down hill, as they would do if they had been produced by glaciers, but north and south horizontally along the slopes, in a manner that might have been produced by bergs grating along the coast during submergence. These striations were observed to reach the height of 2850 feet above the sea. In the gorge, where the hotel stands at that height, they turn sharply round, trending nearly east and west; as if at a certain period of submergence, the floating ice had been at liberty to pass across its ordinary course in a strait between two islands. During the greatest amount of submergence of the country, the glacial sea in the valley of the Hudson must have been between 3000 and 4000 feet deep, and it is probable that even the highest tops of the Catskills lie below the water.

In Wales, it has been shown that during the emergence of the country in the glacial epoch, the drift in some cases was ploughed out of the valleys by glaciers; but though the Catskill mountains

are equally high, in the valleys beyond the great eastern escarpment the drift still exists, which would not have been the case had glaciers filled these valleys during emergence in the way that took place in the Passes of Llanberis and Nant-Francon, and in parts of the Highlands of Scotland.

It has been stated above that the upper plain around Lake Erie, and the lower plain of Lake Ontario, are alike covered with drift. Part of this was formed, and much of it modified during the emergence of the country. In the valley of the St. Lawrence, near Montreal, about 100 feet above the river, there are beds of clay, containing Leda Portlandica, and called by Dr. Dawson of Montreal, the *Leda clay*. Dr. Dawson is of opinion that when this clay was formed, the sea in which it was deposited washed the base of the old coast line that now makes the great escarpment at Queenston and Lewiston, overlooking the plains round Lake Ontario. It has long been an accepted belief that the Falls of Niagara commenced at the edge of this escarpment, and that the gorge has gradually been produced by the river wearing its way back for seven miles to the place of the present Falls.* In this case, the author conceives that *the Falls commenced during the deposition of the Leda clay, or near the close of the drift period*, when during the emergence of the country the escarpment had already risen partly above water. If it should ever prove possible to determine the actual rate of recession of the Falls, we shall thus have data by which to determine approximately the time that has elapsed since the close of the drift period; and an important step may thus be gained towards the actual estimate of a portion of geological time.

[A. C. R.]

ANNUAL MEETING,

Saturday, May 1.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S., President, in the Chair.

The Annual Report of the Committee of Visitors was read, and adopted.

The statement of Sums Received shows a steady and gradual

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* The details on which this belief is founded, may be found in the writings of Professor Hall, of Albany, and Sir Charles Lyell.

increase in the yearly income. The amount of Annual Contributions for 1857 amounted to £2006. 11*s.* 0*d.*, being more than had been received in any previous year: while the Receipts from Subscriptions to Lectures (£761. 15*s.* 6*d.*) were greater than in any of the three preceding years, or than the average receipt from that source in the last ten years.

During the last ten years the Members and Annual Subscribers have increased from 328 to 427, being an addition of nearly one-third.

On Dec. 31, 1857, the Funded Property was £25,166. 5*s.* 10*d.*; and the Balance in the hands of the Bankers, £930. 13*s.* 8*d.* There were no Liabilities.

A List of Books Presented accompanies the Report, amounting in number to 264 volumes, and making, with those purchased by the Managers and Patrons, a total of 1009 volumes (including Periodicals) added to the Library in the year.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to Professor Faraday, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:—

PRESIDENT—The Duke of Northumberland, K.G. F.R.S.
TREASURER—William Pole, Esq. M.A. F.R.S.
SECRETARY—Rev. John Barlow, M.A. F.R.S.

MANAGERS.

The Lord Ashburton, D.C.L. F.R.S.
Warren De la Rue, Esq. Ph.D. F.R.S.
George Dodd, Esq. F.S.A.
Sir Charles Fellows, F.G.S.
William Robert Grove, Esq. M.A. Q.C. F.R.S.
Sir Charles Hamilton, Bart. C.B
Sir H. Holland, Bt. M.D. F.R.S. F.G.S.
Henry Bence Jones, M.D. F.R.S.
Sir Roderick I. Murchison, G.C.S. D.C.L. F.R.S.
James Rennie, Esq. F.R.S.
Robert P. Roupell, Esq. M.A. Q.C.
Rev. William Taylor, F.R.S.
John Webster, M.D. F.R.S.
Charles Wheatstone, Esq. F.R.S.
Col. Philip James Yorke, F.R.S.

VISITORS.

Allen Alexander Bathurst, Esq. M.P.
John Charles Burgoyne, Esq.
John Robert F. Burnett, Esq.
C. Wentworth Dilke, jun. Esq.
William Gaussen, Esq.
John Hall Gladstone, Esq. Ph.D. F.R.S.
Thomas Lee, Esq.
Charles Lyall, Esq.
Thomas N. R. Morson, Esq.
Sir Edwin Pearson, M.A. F.R.S.
Henry Pemberton, Esq.
James Rennell Rodd, Esq.
William Roxburgh, M.D.
Joseph Skey, M.D.
John Godfrey Teed, Esq. Q.C.

The President nominated the following Vice-Presidents for the ensuing year :—

William Pole, Esq. *the Treasurer.*
Rev. John Barlow, *the Secretary.*
The Lord Ashburton.
William Robert Grove, Esq.
Sir Roderick I. Murchison.
Charles Wheatstone, Esq.

GENERAL MONTHLY MEETING,

Monday, May 3.

William Pole, Esq. M.A. F.R.S. Treasurer and Vice-President, in the Chair.

Colonel Edward Francis Grant.
Francis Hird, Esq. F.G.S.
George Kingsley, M.D.
Sir Henry Charles Paulet, Bart.
Robert Parris, Esq.
Sir Charles Taylor, Bart., and
Christopher Welch, Esq.

were duly *elected* Members of the Royal Institution.

The following Professors were re-elected :—

William Thomas Brande, Esq. D.C.L. F.R.S. as Honorary Professor of Chemistry.

John Tyndall, Esq. F.R.S. as Professor of Natural Philosophy.

Actonian Prize.—The Managers reported, That in their judgment no Essay had been received by them within the period of seven years since the last award in 1851, of sufficient merit to entitle the author thereof to the prize of £105; that consequently no prize had been awarded this year; and that the £105 intended to have been awarded, would, pursuant to the Trust-Deed, be retained and awarded with another sum of £105 in 1865, of which due notice would be given.

The Managers further reported, That the late Richard Horsman Solly, Esq. M.R.I. had bequeathed by his will £100 to the Royal Institution.

The following PRESENTS were announced, and the thanks of the Members returned for the same :—

FROM

Actuaries, Institute of—Assurance Magazine, No. 31. 8vo. 1858.

Astronomical Society, Royal—Monthly Notices, April, 1858; and Vol. XVII. 8vo. 1856–7.

Memoirs, Vol. XXVI. 4to. 1857.

Barlow, Rev. John, M.A. F.R.S. V.P. and Sec. R.I.—Jo. B. Portæ Magiæ Naturalis Libri xx. 8vo. Rothomagi, 1650.

Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for April 1858. 8vo.

Boosey, Messrs. (the Publishers)—The Musical World for April 1858. 4to.

Botfield, Beriah, Esq. M.P. M.R.I. (the Author)—Catalogue of the Minister's Library in the Collegiate Church of Tong, in Shropshire. 4to. 1858.

British Architects, Royal Institute of—Proceedings in April 1858. 4to.

Chemical Society—Quarterly Journal, No. 41. 8vo. 1858.

Editors—The Medical Circular for April 1858. 8vo.

The Practical Mechanic's Journal for April 1858. 4to.

The Journal of Gas-Lighting for April 1858. 4to.

The Mechanics' Magazine for April 1858. 8vo.

The Athenæum for April 1858. 4to.

The Engineer for April 1858. fol.

The Artizan for April 1858. 4to.

Faraday, Professor, D.C.L. F.R.S.—Königliche Preussischen Akademie; Berichte, Jan. 1858. 8vo.

Franklin Institute of Pennsylvania—Journal, Vol. XXXIV. No. 3. 8vo .1858

Geographical Society, Royal—Proceedings, Vol. II.No. 2. 8vo. 1858.

Journal, Vol. XXVII. 8vo. 1857.

Geological Society—Proceedings, April 1858. 8vo.

Graham, George, Esq. (Registrar-General)—Report of the Registrar-General for April 1858. 8vo.

Hall, Spencer, Esq.—Dr. Maitland's Notes on Strype. 8vo. 1858.

Hosking, W. Esq. (the Author)—Observations on the New Reading Room in the British Museum. fol. 1858.

Newton, Messrs.—London Journal (New Series), April 1858. 8vo.

Novello, Mr. (the Publisher)—The Musical Times, for April 1858.

Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie. 1858. Heft 2. 4to. Gotha, 1858.

Photographic Society—Journal, No. 65. 8vo. 1858.

Poey, M. Andrés (the Author)—Sur la Systématisation des Phénomènes Météorologiques, &c. (3 Pamphlets.) 8vo. 1857–8.

Prytherch, Dr.—Apollyon, and the Reaction of the Slavonians. By Col. F. T. Buller. 8vo. 1847.

Roupell, Mrs. T. B. (the Author)—Specimens of the Flora of South Africa. (Plates.) fol. 1849.

Dublin Society, Royal—Journal, Nos. 7 and 8. 8vo. 1858.

Royal Medical and Chirurgical Society—Proceedings, Vol. I.; and Vol. II. No. 1. 8vo. 1857–8.

Royal Society—Proceedings, No. 30. 8vo. 1858.

Society of Arts—Journal for April 1858. 8vo.

Vereins zur Beförderung des Gewerbfleisses in Preussen—Jan. und Feb. 1858. 4to.

Vincent, B. Assist.-Sec. R.I.—Odes of Hafiz, in Persian. MS.

Warburton, Henry, Esq. F.R.S. M.R.I.—The New Charter of the Universit of London. 8vo. 1858.

WEEKLY EVENING MEETING,

Friday, May 7.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

James Philip Lacaita, Esq. LL.D.

On the late Earthquakes in Southern Italy.

Southern Italy is celebrated for its delightful climate, its matchless scenery, its great historical associations; but it has also a less enviable renown; it is the classic ground of volcanoes and earthquakes. Etna and Vesuvius are the two most active volcanoes in Europe, and terrific earthquakes have often desolated vast districts of the country.

Though the common origin, to a certain extent, of the agents producing the phenomena of volcanoes and earthquakes is now scarcely questioned, considerable difference of opinion still prevails with regard to the real nature and character of those agents. It is for men of science to determine whether those agents are to be found in the internal heat of the earth which is supposed to arise from a state of fusion; or in the heat produced by chemical combinations and changes; or in the currents of electricity circulating on the earth's crust; or in any other causes whatsoever. On this *vexata quæstio* much light will no doubt be thrown before long by the observations made on the spot by Mr. Mallet, the distinguished author of the "Dynamics of Earthquakes," who, on the first news of the late earthquake in Southern Italy in December last, was sent thither by the Royal Society, for the pursuit of scientific enquiry. Without entering, however, into the field of science, the object of the speaker was to give the members of the Royal Institution a short account of six great earthquakes, without counting minor ones, which within the memory of man laid waste extensive tracts of the kingdom of Naples and caused great loss of life; and especially of the last earthquake, which took place on the night of the 16th of December, 1857.

1. On the 5th of February, 1783, at 1 p.m., the Piana di Monteleone, in the province of Calabria Ultra I., was convulsed by a violent shock of earthquake, which in less than two minutes levelled to the ground 109 towns and villages, and buried 32,000 out of 166,000 inhabitants under the ruins of their houses. A

repetition of the shock at midnight ruined the towns of Reggio and Messina, and convulsed the whole Valdemone. At the entrance of the Faro Straits, the sea, retiring from the Calabrian shore and afterwards rushing back with overwhelming violence, swept away more than 1500 inhabitants of the town of Scylla, who had taken refuge on the beach for safety. After a succession of slight shocks, on the 28th of the following March, another violent shock convulsed the whole country from Reggio to Cape Colonna, an area of 1200 square miles, and added two thousand more to the number of victims. Mountains were cleft asunder, high cliffs tumbled down, rivers turned from their bed or dammed in their course, lakes formed, valleys lifted up into hills, deep chasms opened, the physical aspect of the country changed, all distinctions of property altered. For twenty days a thick pestilential fog set over the desolated country; epidemic fevers followed in summer; and at the beginning of 1784 Calabria had already lost more than 80,000 inhabitants. From February to December 1783, there were no less than 949 shocks, and 151 in 1784; they did not altogether cease till 1786.

2. The mountain of Frosolone, in the province of Molise, the ancient *Samnium*, on the 26th of July, 1804, at 10½ p.m., was the centre of a violent shock of earthquake, which lasted 35 seconds, and caused great desolation over an area of 600 square miles. It ruined 61 towns and villages, and crushed to death more than 6000 people. It was severely felt as far as Naples, where all the buildings were greatly injured by its effects.

3. On the 29th of April, 1835, and on several successive days, the Val di Crati, in the province of Calabria Citra, including the town of Cosenza and its numerous villages, was convulsed by violent shocks of earthquake, which caused the death of more than 1000 people under the ruins.

4. On the 12th of October, 1836, the districts of Rossano and Castrovillari, in the same province, and the district of Lagonegro, in Basilicata, felt another violent shock of earthquake, which swept away more than 600 inhabitants.

5. The city of Melfi, built on a spur of Mount Vulture, an extinct volcano in the province of Basilicata, on the 14th of August 1851, was the focus of a violent earthquake, which, besides Melfi itself, ruined Barile, Rapolla, and many other towns, and was felt as far as Naples on the western, and Brindisi on the eastern coast. The first shock, at 2 p.m., lasted 20 seconds; the second shock, at 3 p.m., lasted only five seconds. The loss of human life exceeded 1400; Melfi alone, out of 9274, lost 1093 inhabitants.

6. But worse than any of the later earthquakes, and second only to the Calabrian one of 1783, was the earthquake which took place on the 16th of December last,* at 10¼ p.m., at a season of the year, which, by a comparison of all the known dates of earthquakes,

has been ascertained to be more subject to disturbances than any other. The sky was clear, the air still; indeed unusual stillness had prevailed the whole of that day. A sharp undulatory shock of 20 seconds' duration, immediately preceded and accompanied by an appalling hollow rumbling noise, had scarcely awaked the inhabitants, who, according to the early habits of provincial life had already retired to rest, when after a hardly perceptible pause of about three minutes, a second and most violent successive and whirling shock of 25 seconds' duration crushed thousands of them under the ruins of their falling houses. Three other shocks were felt on that awful night, and many others on the following days; but none nearly so violent and so destructive as the two former ones. For nearly two months a slight shock was felt almost periodically just before sunrise. On the 7th of March, about 3 p.m., a violent shock, second only to those of the 16th of December, was felt, which caused considerable injury; and, according to the latest accounts, up to the 28th of April last, the shocks, though comparatively slight and harmless, still continued, and the people were in a state of constant alarm. Such was also the case in every one of the five previous earthquakes that have been noticed; the violence of the hidden agents at work was not at once exhausted by the first great shocks, but continued slightly to shake the ground for months, and sometimes, as in the Calabrian earthquake of 1783, for nearly four years afterwards.

The seat of this earthquake was in the central group of mountains in the provinces of Basilicata and Principato Citra, part of the main chain of the Apennines, which are the watershed between the streams flowing into the Tyrrhenian, the Ionian, and the Adriatic sea, and form the upper basins of the Calore or Tanagro, the Sele, the Ofanto, the Bradano, the Basento, the Sinno, and the Agri rivers. The centre of action, as far as it can be judged from the intensity of its terrific effects, was almost in the heart of the province of Basilicata, in a group of compact limestone mountains of the cretaceous period, the southern branch of the said central group, which running from north to south between the heads of the valleys of the Sinno and the Agri on the east, and the valley of Diano on the west, swells farther south into the lofty peaks of Monte Cocuzzo, Monte del Papa, and Monte Pollino, on the frontiers of Calabria. On the declivities or lower peaks of this group, which are covered with beds of tertiary marine marl sands and conglomerate, and within a district extending over an area of about 216 square miles, stand, or rather stood, the towns and villages of Montemurro, Saponara, Viggiano, Tramutola, Marsico Vetere, Marsico Nuovo, Spinosa and Sarconi, with an aggregate population of 35,570. Out of this number more than 12,000, or more than one-third, in less than half a minute were crushed to death; two thousand severely wounded! The ground was cracked and convulsed in the strangest manner; chasms and deep fissures were opened in several places,

fertile hills became bare rocks, valleys were raised up, small pools formed, mountains cleft by deep ravines. The towns of Montemurro and Saponara especially were nearly entirely swept away; the former lost 5600 out of 7000, and the latter 3000 out of 4000 inhabitants. Saponara, which rose in the middle ages out of the ancient *Grumentum*, where Hannibal sustained a slight defeat by the Consul Claudius Nero, was almost entirely levelled with the ground; there remain only a few shattered houses standing. Of Montemurro, originally a Saracenic settlement of the tenth century, literally nothing was left but a heap of rubbish. On the morning of the 17th of December, 5600 of its inhabitants were dead or dying under the ruins, 685 disabled by wounds; the few remaining unhurt found themselves torn from their dearest ones, houseless, amidst a mass of ruins, without means of subsistence or help, and exposed to all the inclemency of a severe winter on a high peak of the Apennines! A few days later the stench of the dead human beings under the ruins made life unbearable to the few surviving ones! Both at Montemurro and Saponara, most of the houses standing on beds of conglomerate had been overturned, or shuffled in the strangest manner, and the ruins deposited in the ravines beneath; the contents of the lower stories were, in several instances, thrown up into the stories above, or scattered into different directions, as if propelled by a central force. The scenes of misery and horror that took place in those doomed towns exceed what imagination can fancy. Viggiano came next, a town whose inhabitants from time immemorial have been in the habit of wandering, with their harps over different parts of the world, and return home with their savings in summer. It lost 1700 out of 6634 inhabitants, and had most of the houses and churches overthrown. At this place an extensive fire added to the horrors of the night.

From the centre of a triangle formed by these three towns, on which the fury of the convulsion was more violently wreaked, the distances, in a direct line, are,—to the Gulf of Policastro, 24 miles; to Pæstum, on the Gulf of Salerno, 58 miles; to the mouth of the Agri, on the Gulf of Tarentum, 47 miles; to the extinct volcano of Mount Vulture, 55 miles; to Mount Vesuvius, 94 miles; to Bari, on the Adriatic, 80 miles; and to Mount Etna, 195 miles.

Beyond this district, the terrific effects of the earthquake extended, though somewhat diminished in intensity, over an area of more than 3000 square miles, destroying or injuring, more or less, about 200 towns and villages, with an aggregate population of more than 200,000 inhabitants, of whom no less than 10,000 were killed.

Within this area the beautiful and fertile valley of Diano, through which flows the Tanagro, a tributary of the Sele, traversed in its length by the high road leading into Calabria, and enlivened on both sides by numerous towns and villages built on the top or

the slope of the hills, was sadly desolated. Polla is said to have lost 2000 out of 7060 inhabitants; Padula, 500 out of 9000; Pertosa, 218 out of 1100; Sassano, 185 out of 3600; Montesano, 420 out of 4800, &c. Leaving the valley of Diano, and proceeding northwards to the head of the valley of the Sele, will be found Brienza, Calvello, St. Angelo Le Fratte, Picerno, Tito, Potenza, the capital of Basilicata, &c., with most of their houses and public buildings ruined, and many of their inhabitants killed. At Tito, in particular, more than 300 out of 4939 inhabitants were crushed to death, and its beautiful Norman cathedral totally thrown to the ground. South of Potenza, in the upper valleys of the Bradano, the Basento and the Agri, and eastward of the centre of action, Laurenzana, Corleto, Guardia, Aliano, Armento, Gallicchio, Missanello, Sant' Arcangelo, Castelsaraceno, and numerous other towns and villages, had most of the houses thrown down, and many inhabitants killed.

But the effects of this terrific earthquake extended far beyond the large area that has just been noticed. The two shocks of the 16th were felt, with various degrees of intensity, as far as the town of Reggio in Calabria on the south, Brindisi on the Adriatic, on the east, Vasto, also on the Adriatic, on the north, and Terracina on the west. Within these limits many towns had their buildings much injured, and some inhabitants killed. All the towns on the Adriatic, from Poliguano to Manfredonia, had their buildings rent. At Canosa, 15 houses were thrown down, 155 more rendered uninhabitable, and 5 persons were killed. At Melfi and Barile, there were three deaths. In the neighbourhood of Bella, a town which stands half way between Potenza and Melfi, a tract of about 600 acres was split in different directions, and surrounded with a chasm 15 feet deep, and about as wide. At Salerno, many public buildings were injured, and 4 persons killed. Even at Tramonti, near Amalfi, there were two deaths; and at Naples, the inhabitants were so greatly alarmed by the violence of the shocks, as to spend in the open air all the night of the 16th of December.

On the whole, by this terrific earthquake, at least 22,000 human beings, on a most moderate calculation, were destroyed in a few seconds. Many no doubt would have been saved had it been possible by active steps to dig them out immediately. This will account for the comparatively very small number of wounded, in all about 4000.

From the above data it will be seen that in the course of 75 years, from 1783 to 1857, the kingdom of Naples lost at least 111,000 inhabitants, by the effects of earthquakes, or more than 1500 per year, out of an average population of six millions!

Several touching anecdotes were told in the course of the narrative. In 1783, Eloisa Basili, a beautiful girl of 16, was buried under the ruins with a child in her arms, who died on the fourth day. She was so wedged in that she could not get rid of its lifeless

remains. She was dug out alive after eleven days, which she had counted from a ray of light that reached her. She recovered, but remained sad and gloomy, could not bear to see a child, would neither marry nor become a nun. She preferred solitude, turned away with a shudder from houses, and liked to sit musing under a tree, whence no buildings were seen. She pined away, and died at five-and-twenty.

More fortunate was the lot of Marianna De' Franceschi, a beautiful young lady of 20, who, in the earthquake of 1804, was dug out at Guardia Regia, after being buried for ten days and eight hours. She recovered, married, and became the mother of a numerous family.

A lady with child was dug out after 30 hours by her devoted husband, who nearly died from over-fatigue. On being asked what her thoughts were during the time, she answered, "I was waiting."

In the late earthquake, a gentleman of Montemurro, whilst escaping from the house with his wife and a large family of children, remembered that one of them had been left in bed. He rushed back to take him, but the house tumbling on every side, he remained alone on a wall. All his family were crushed to death. The blow was too great; his mind gave way, and he went raving mad. At Saponara, the judge was buried under the ruins of his house with his young wife and two children. He was dug out alive, but his wife was found dead lying across his knees with her arms outstretched towards her dead children. He was overwhelmed by his loss; ever since he has diligently fulfilled the duties of his office, but has never been heard to allude to the event, or seen to smile.

Instances were mentioned, showing how tenacious life could be under the most trying circumstances. Besides the cases of Basili and De' Franceschi already recorded, in 1783 a baby was dug out alive on the third day, and lived. At Montemurro, in December last, Maria Antonia Palermo and her two little girls, one of them only thirteen months old, were dug out on the eighth day, and lived. With some animals the length of time they had stood alive was quite remarkable. A donkey was found living yet on the fifteenth day; and in 1783 two mules and a chicken were found still alive on the twenty-second, and two pigs on the thirty-second day.

Five photographs of some of the ruined towns, which Mr. Mallet had kindly lent for the evening, were exhibited. Of the ruined cathedral of Tito, and of the churches of Polla, and other ruins, beautiful illustrations were afforded at the end of the discourse, by a series of photographs which Messrs. Negretti and Zambra, of Hatton Garden, had had executed on the spot, and which, by the aid of the electric lamp, were reproduced on a large scale on the wall.

[J. P. L.]

WEEKLY EVENING MEETING,

Friday, May 14.

The Rev. John Barlow, M.A. F.R.S. Vice-President and Secretary, in the Chair.

Henry Bradbury, Esq. M.R.I.

Printing; its Dawn, Day, and Destiny.

[*No Abstract received.*—The Discourse has been printed in full, and a copy presented to the Library of the Royal Institution.]

WEEKLY EVENING MEETING,

Friday, May 21.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Thomas H. Huxley, F.R.S.

FULLERIAN PROFESSOR OF PHYSIOLOGY ROYAL INSTITUTION, AND PROFESSOR OF NATURAL HISTORY, GOVERNMENT SCHOOL OF MINES, JERMYN STREET.

On the Phenomena of Gemmation.

The speaker commenced by stating that a learned French naturalist, M. Duvau, proposed many years ago, to term the middle of the eighteenth century, "l'Epoque des Pucerons:" and that the importance of the phenomena which were first brought to light by the study of these remarkable insects renders the phrase "Epoch of Plant-lice," as applied to this period, far less whimsically inappropriate than it might at first sight seem to be.

After a brief sketch of the mode of life of these Plant-lice, or *Aphides*, as they are technically termed; of the structure of their singular piercing and sucking mouths; and of their relations to what are called "Blights;" the circumstances which have more particularly drawn the attention of naturalists to these insects were fully detailed.

It was between the years 1740 and 1750, in fact, that Bonnet, acting upon the suggestions of the illustrious Reaumur, isolated an *Aphis* immediately after its birth, and proved to demonstration, that not only was it capable of spontaneously bringing forth numerous living young, but that these and their descendants, to the ninth generation, preserved a similar faculty.

Observations so remarkable were not likely to pass unheeded; but notwithstanding the careful sifting which they have received, Bonnet's results have never been questioned. On the contrary, not only have Lyonet, Degeer, Kyber, Duvau, and others, borne ample testimony to their accuracy, but it has been shown that, under favourable conditions of temperature and food, there is practically no limit to this power of asexual multiplication, or as it has been conveniently termed, "Agamogenesis."

Thus Kyber bred the viviparous *Aphis Dianthi* and *Aphis Rosæ* for three years in uninterrupted succession; and the males and true oviparous females of the *A. dianthi* have never yet been met with. The current notion that there is a fixed number of broods, "nine or eleven," is based on a mistake.

As, under moderately favourable conditions, an *Aphis* comes to maturity in about a fortnight; and as each *Aphis* is known to be capable of producing a hundred young, the number of the progeny which may eventually result even from a single *Aphis* during the six or seven warm months of the year is easily calculated. M. Tougard's estimate adopted, (and acknowledged) by Morren, and copied from him by others, gives the number of the tenth brood as one quintillion. Supposing the weight of each *Aphis* to be no more than $\frac{1}{1000}$th of a grain, the mass of living matter in this brood would exceed that in the most thickly populated countries in the world.

The agamogenetic broods are either winged or wingless. The winged forms at times rise into the air, and are carried away by the wind in clouds; and these migrating hordes have been supposed to be males and females, swarming like the ants and bees! During the summer months it is unusual to meet other than viviparous *Aphides*, whether winged or wingless; but ordinarily, on the approach of cold weather, or even during warm weather, if the supplies of food fall short, the viviparous *Aphides* produce forms which are no longer viviparous, but are males and oviparous females. The former are sometimes winged, sometimes wingless. The latter, with a single doubtful exception, are always wingless.

The oviparous females lay their eggs, and then, like the males, die. It commonly happens also that the viviparous *Aphides* die, and then the eggs are left as the sole representatives of the species; but in mild winters many of the viviparous *Aphides* merely fall into a state of stupor and hybernate, to re-awake with the returning warmth of spring. At the same time the eggs are hatched and give rise to viviparous *Aphides*, which run through the same course as before.

The species *Aphis*, therefore, is fully manifested not in any one being or animated form, but by a cycle of such, consisting of,—1st the egg; 2nd, An indefinite succession of viviparous *Aphides;* 3rd, Males and females eventually produced by these, and giving rise to the egg again.

If, armed with the microscope and scalpel, we examine into the minute nature of these processes (without which inquiry all speculation upon their nature is vain), we find that the viviparous *Aphis* contains an organ similar to the ovarium of the oviparous female, in some respects, but differing from it, as Von Siebold was the first to show, in the absence of what are termed the colleterial glands and the spermatheca—organs of essential importance to the oviparous form.

In the terminal chambers of this "Pseud-ovarium," ovum-like bodies, thence called "pseud-ova," are found. These bodies pass one by one into the pseudovarian tubes, and there gradually become developed into young, living *Aphides*. As Morren has well said, therefore, the young *Aphides* are produced by "the individualization of a previously organized tissue."

The only organic operation with which this mode of development can be compared, is the process of budding or gemmation, as it takes place in the vegetable kingdom, in the lower forms of animal life, and in the process of formation of the limbs and other organs of the higher animals. And the parallel is complete if such a plant as the bulbiferous lily or the *Marchantia*, or such an animal as the *Hydra*, is made the term of comparison.

Thus agamogenesis in *Aphis*, is a kind of internal budding or gemmation. If we inquire how this process differs from multiplication by true ova or "Gamogenesis," we find that the young ovum in the ovarium is also, to all intents and purposes, a bud, indistinguishable from the germ in the pseudovarium of the agamogenetic *Aphis*. Histologically there is no difference between the two; but there is an immense qualitative or physiological difference, which cannot be detected by the eye, but becomes at once obvious in the behaviour of the two germs after a certain period of their growth. Dating from this period, the pseudovum spontaneously passes into the form of an embryo, becoming larger and larger as it does so; but the ovum simply enlarges, accumulates nutritive matter, acquires its outer investments, and then falls into a state of apparent rest, from which it will never emerge, unless the influence of the spermatozoon have been brought to bear upon it.

That the vast physiological difference between the ovum and the pseudovum should reveal itself in the young state by no external sign, is no more wonderful than that primarily the tissue of the brain should be undistinguishable from that of the heart.

The phenomena which have been described, were long supposed to be isolated, but numerous cases of a like kind, some even more remarkable, are now known.

Among the latter, the speaker cited the wonderful circumstances attending the production of the drones among bees, as described by Von Siebold; and he drew attention to the plant upon the table, *Cœlobogyne ilicifolia*, a female euphorbiaceous shrub, the male flowers of which have never yet been seen, and which nevertheless, for the last twenty years, has produced its annual crop of fertile seeds in Kew Gardens.

Not only can we find numerous cases of agamogenesis similar to that exhibited by *Aphis* in the animal and vegetable worlds, but if we look closely into the matter, agamogenesis is found to pass by insensible gradations into the commonest phenomena of life. All life, in fact, is accompanied by incessant growth and metamorphosis; and every animal and plant above the very lowest attains its adult form by the development of a succession of buds. When these buds remain connected together, we do not distinguish the process as anything remarkable; when on the other hand, they become detached and live independently, we have agamogenesis. Why some buds assume one form and some another, why some remain attached and some become detached, we know not. Such phenomena are for the present the ultimate facts of biological science; and as we cannot understand the simplest among them, it would seem useless, as yet, to seek for an explanation of the more complex.

Nevertheless, an explanation of agamogenesis in the *Aphis* and in like cases has been offered. It has been supposed to depend upon "the retention unchanged of some part of the primitive germ-mass;" this germ-mass being imagined to be the seat of a peculiar force, by virtue of which it gives rise to independent organisms.

There are however two objections to this hypothesis: in the first place, it is at direct variance with the results of observation; in the second, even if it were true, it does not help us to understand the phenomena. With regard to the former point, the hypothesis professes to be based upon only two direct observations, one upon *Aphis*, the other upon *Hydra;* and both these observations are erroneous, for in neither of these animals is any portion of the primitive germ-mass retained, as it is said to be, in that part which is the seat of agamogenesis.

But suppose the fact to be as the hypothesis requires; imagine that the terminal chamber of the pseudovarium is full of nothing but "unaltered germ-cells;" how does this explain the phenomena? Structures having quite as great claim to the title of "unaltered germ cells" lie in the extremities of the acini of the secreting glands, in the sub-epidermal tissues and elsewhere; why do not they give rise to young? Cells, less changed than those of the pseudovarium of *Aphis*, and more directly derived from the primitive germ-mass, underlie the epidermis of one's hand; nevertheless, no one feels any alarm lest a nascent wart should turn out to be an heir.

On the whole, it would seem better, when one is ignorant, to say so, and not to retard the progress of sound inquiry by inventing hypotheses involving the assumption of structures which have no existence, and of "forces" which, their laws being undetermined, are merely verbal entities.

[T. H. H.]

WEEKLY EVENING MEETING,

Friday, May 28.

William Robert Grove, Esq. M.A. Q.C. F.R.S. Vice-President, in the Chair.

Professor E. Frankland, F.R.S. F.C.S.

LECTURER ON CHEMISTRY AT ST. BARTHOLOMEW'S HOSPITAL.

On the Production of Organic Bodies without the agency of Vitality.

The earlier researches of chemists brought them into contact with two classes of bodies, distinguished from each other by well-marked and obvious peculiarities. One of them was met with in the inanimate or mineral kingdom, the various materials of which were distinguished by their comparative stability or resistance to change, and by the facility with which the greater number of them could be artificially produced from the elementary bodies composing them. The other class of bodies was found exclusively in the animate portion of creation, or was directly derived from the productions of the organs of plants and of animals: these compounds were distinguished by their proneness to undergo change, and by the impossibility of producing them by artificial means. By no processes then known to chemists, could the elements composing these latter bodies be made to unite, so as to produce compounds, either identical with, or analogous to, the substances generated by the organs of plants and of animals. These substances were consequently, from their origin, termed *organic bodies* or *organic compounds.* They were regarded as dependent for their origin, upon the influence of that aggregate of conditions sometimes called *vital force;* and it was generally believed, that we should never succeed in producing these bodies artificially, until we could form, and endow with vitality, the organs from which they were derived.

Such was the state of knowledge and opinion until the year 1828, when Wöhler succeeded in artificially producing *urea*, a body

which had up to that time been known only as a product of the animal organism.* This discovery was followed many years later by the artificial formation of acetic acid, which was produced by Kolbe from a mixture of protochloride of carbon, water, and chlorine exposed to sunlight, the chloracetic acid thus obtained being afterwards converted into acetic acid by an amalgam of potassium. The subsequent production of methyl by the same chemist from acetic acid, added one of the organic radicals to the list of compounds producible from their elements. Although little further progress was made for several years in this department of chemical research, yet the artificial production of urea and acetic acid, together with their derivatives, completely broke down the barrier between so-called "organic" and "in-organic" bodies; and although the name "organic" was still retained for the class of bodies to which it had previously been assigned, it was now obviously no longer strictly applicable.

The recent ingenious researches of M. Berthelot have greatly extended this branch of chemical enquiry, and have, in a most important degree, increased the number of bodies capable of artificial formation. The production of chloride of methyl and the members of the olefiant gas family up to amylene ($C_{10}\,H_{10}$) furnish us with the whole series of alcohols and their derivatives, from amylic alcohol downwards. Phenylic alcohol and naphthaline, both artificially produced by Berthelot, yield a host of interesting bodies; whilst phenylcarbamic acid enables us to step from the phenylic to the salicylic group, since, when treated with hyponitrous acid, it yields salicylic acid. Lastly, M. Berthelot has succeeded in artificially forming glycerine, the basis of animal and vegetable oils and fats, and also in forming grape sugar; the latter however is obtained by the contact of glycerine with putrifying animal matter, and consequently cannot be said to be produced altogether without the agency of vitality; although the putrifying organic matter contributes none of its constituents to the new compound, and does not undergo any appreciable change in weight or appearance during the process. These substances yield such a numerous class of derivatives that upwards of 700 distinct organic compounds can now be produced from their elements without the agency of vitality.

The processes employed for the artificial production of these bodies, though deeply interesting, present, with one or two exceptions, little or no analogy to the natural mode by which organic compounds are formed in the tissues of plants; but the speaker endeavoured to show, that a close attention to the nature of the inorganic materials assimilated by the vegetable kingdom, and their relations to the more important organic compounds derived from plants, leads to the belief, that such compounds can be successfully

* The artificial formation of urea from cyanate of ammonia was exhibited under the influence of polarised electric light.

produced by processes strictly analogous to those employed by nature. He contended that the constitution of the so-called organo-metallic bodies, in which the production of complex organic compounds from inorganic ones by the replacement of elements by organic groups, can be so clearly traced, afforded a valuable clue to the formation of organic bodies in general, and led directly to the conclusion, that if the organic compounds of the metals be formed upon the model of the oxides of the respective metals, the organic compounds of carbon (that is, all organic compounds) are formed upon the model of the oxides of carbon.

It has long been known, that with slight and unimportant exceptions, the only materials employed by nature in the construction of the most complex organic compounds, are carbonic acid, water, ammonia, and nitric acid. The fact that a vast number of organic compounds are cast in the molecular mould of water, has been proved by the ingenious researches of Williamson and Gerhardt; whilst the wonderful fertility of the ammonia model has been amply demonstrated by the labours of Hofmann and Wurtz. It would also not be difficult, to prove the claim of nitric acid to be considered as a third model, upon which a number of other organic compounds are built up; but it was necessary to confine attention on the present occasion to the consideration of carbonic acid only, as a model upon which a very large number of organic bodies are formed.

Guided by the constitution above referred to, of the organo-metallic bodies, and bearing in mind the replacibility of the oxygen in water and binoxide of nitrogen, and the chlorine in terchloride of phosphorus, by organic radicals; Professor Kolbe and the speaker were led to the following hypothesis regarding the constitution of several important classes of organic compounds.

1. The replacement of one atom of oxygen in carbonic acid by hydrogen, or its homologues, produces an organic acid, either of the fatty or of the aromatic series, thus:—

$$\begin{array}{ccc} \text{Carbonic Acid.} & \text{Acetic Acid.} & \text{Benzoic Acid.} \\ C_2\left\{\begin{array}{l} O \\ O \\ O \\ O \end{array}\right. & C_2\left\{\begin{array}{c} (C_2\,H_3) \\ O \\ O \\ O \end{array}\right. & C_2\left\{\begin{array}{c} (C_{12}\,H_5) \\ O \\ O \\ O \end{array}\right. \end{array}$$

2. The like replacement of two atoms of oxygen in carbonic acid, produces either a ketone, or an aldehyde, thus:—

$$\begin{array}{cccc} \text{Carbonic Acid.} & \text{Acetone.} & \text{Aldehyde.} & \text{Oil of Bitter Almonds.} \\ C_2\left\{\begin{array}{l} O \\ O \\ O \\ O \end{array}\right. & C_2\left\{\begin{array}{c} (C_2\,H_3) \\ (C_2\,H_3) \\ O \\ O \end{array}\right. & C_2\left\{\begin{array}{c} C_2\,H_3 \\ H \\ O \\ O \end{array}\right. & C_2\left\{\begin{array}{c} C_{12}\,H_5 \\ H \\ O \\ O \end{array}\right. \end{array}$$

3. The like replacement of three atoms of oxygen in carbonic acid, produces an ether, thus :—

$$\text{Carbonic Acid.}\quad C_2\left\{\begin{matrix}O\\O\\O\\O\end{matrix}\right. \qquad\qquad \text{Vinic Ether.}\quad C_2\left\{\begin{matrix}C_2\,H_3\\H\\H\\O\end{matrix}\right.$$

4. The like replacement of all the atoms of oxygen in carbonic acid, produces a radical, a hydride of a radical, or a double radical, thus :—

$$\text{Carbonic Acid.}\quad C_2\left\{\begin{matrix}O\\O\\O\\O\end{matrix}\right. \qquad \text{Ethyl.}\quad C_2\left\{\begin{matrix}C_2\,H_3\\H\\H\\C_4\,H_5\end{matrix}\right. \qquad \begin{matrix}\text{Hydride of Methyl.}\\\text{(Fire damp.)}\end{matrix}\quad C_2\left\{\begin{matrix}H\\H\\H\\H\end{matrix}\right. \qquad \text{Methyl-ethyl.}\quad C_2\left\{\begin{matrix}C_2\,H_3\\H\\H\\C_2\,H_3\end{matrix}\right. .$$

The authors of this hypothesis now attempted to verify it by direct experiment. They endeavoured to avail themselves of the powerful affinities of zinc-ethyl, in order to effect the substitution of oxygen in carbonic acid, and sulphur in bisulphide of carbon, by ethyl; these attempts were, however, at best only partially successful; the re-agent, the zinc-ethyl, was not sufficiently powerful to rival the action of plants in the decomposition of carbonic acid; and its effects upon bisulphide of carbon resulted in the production of a number of organic bodies containing sulphur; and although one of these appeared to have the formula of sulphopropionic acid ($C_6\,H_5\,S_3 + H\,S$), yet its complete separation and purification presented such difficulties, that it would have been hazardous to rely upon it as a proof of the correctness of their hypothesis. In short, the verification of these views was not permitted to their authors, but was reserved for Mr. Wanklyn, who, in his newly-discovered sodium and potassium compounds of the organic radicals, came into possession of re-agents, which enabled him at once to effect the desired substitutions. His memoir on the production of propionic acid by the action of sodium-ethyl upon carbonic acid,* which has just been communicated to the Chemical Society, proves the first proposition of a hypothesis, which considerably simplifies our views of the molecular structure of organic bodies, and which, if proved to be throughout correct, cannot fail to enable us greatly to increase the number of organic compounds capable of being procured from their elements without the intervention of vitality.

The speaker then referred to the following list of important

* This conversion of carbonic acid into propionic acid, was experimentally demonstrated, and the remarkable properties of sodium-ethyl and potassium-ethyl were also exhibited.

organic bodies, selected from the large number above spoken of, as being capable of artificial formation from their elements :—

Name.	Formula.
Oxalic Acid	$(C_2\ O_3,\ H\ O)_2$
Hydrocyanic Acid	$C_2\ N,\ H$
Light Carburetted Hydrogen . . .	$C_2\ H_4$
Urea	$C_2\ N_2\ H_4\ O_2$.
Formic Acid (Acid of Ants) . . .	$C_2\ H\ O_3,\ H\ O$.
Chloroform	$C_2\ H\ Cl_3$.
Acetic Acid	$C_4\ H_3\ O_3,\ H\ O$.
Alcohol	$C_4\ H_5\ O,\ H\ O$.
Ether	$(C_4\ H_5\ O)_2$.
Olefiant Gas	$C_4\ H_4$.
Acetic Ether	$C_4\ H_5\ O,\ C_4\ H_3\ O_3$.
Oil of Garlic	$(C_6\ H_5\ S)_2$.
Oil of Mustard	$C_6\ H_5\ S,\ C_2\ N\ S$.
Glycerine	$C_6\ H_8\ O_6$.
Butyric Acid	$C_8\ H_7\ O_3,\ H\ O$.
Pine Apple flavour (Butyric Ether) .	$C_8\ H_7\ O_3,\ C_4\ H_5\ O$.
Succinic Acid	$C_8\ H_4\ O_6,\ 2\ H\ O$.
Valerianic Acid	$C_{10}\ H_9\ O_3,\ H\ O$.
Pear flavour (Acetate of Amyl) . .	$C_4\ H_3\ O_3,\ C_{10}\ H_{11}\ O$.
Apple flavour (Valerianate of Amyl) .	$C_{10}\ H_9\ O_3,\ C_{10}\ H_{11}\ O$.
Lactic Acid	$C_{12}\ H_{12}\ O_{12}$.
Grape Sugar ?	$C_{12}\ H_{12}\ O_{12}$.
Caproic Acid	$C_{12}\ H_{11}\ O_3,\ H\ O$.
Benzole	$C_{12}\ H_6$.
Nitrobenzole	$C_{12}\ H_5\ N\ O_4$.
Aniline	$N\ (C_{12}\ H_5)\ H_2$.
Phenyl Alcohol (Creosote)	$C_{12}\ H_5\ O,\ H\ O$.
Picric Acid	$C_{12}\ H_2\ (N\ O_4)_3\ O,\ H\ O$.
Salicylic Acid	$C_{14}\ H_5\ O_5,\ H\ O$.
Salicylate of Methyl (Oil of Wintergreen)	$C_{14}\ H_5\ O_5,\ C_2\ H_3\ O$.
Naphthaline	$C_{20}\ H_8$.

The artificial formation of urea, lactic acid, and caproic acid, is interesting in connection with certain functions of the animal economy. Pine-apple oil, pear oil, and apple oil, are instances of the artificial production of the delicate flavours of fruit, whilst oil of wintergreen and nitrobenzole are like examples of the formation of esteemed perfumes. But of all the bodies hitherto thus produced, alcohol, glycerine, and sugar, are undoubtedly the most deeply interesting, owing to the part they take in the nutrition of animals : they prove to us the possibility of producing, without vegetation or any vital intervention, an important part of the food of man. Should the chemist also succeed in forming artificially the nitrogenous constituents of food, without which life cannot be maintained, it would then be possible for a man, placed upon a barren

rock, and furnished with the necessary apparatus and inorganic materials, to support life entirely without either animal or vegetable food. No one of these nitrogenous constituents has however yet been artificially produced, and the absence of all clue to their rational constitution forms at present a formidable barrier to their non-vital formation.

It would be difficult to conclude a subject like the present, without any notice of the considerations which naturally suggest themselves, regarding the possibility of economically replacing natural processes by artificial ones in the formation of organic compounds. At present, the possibility of doing this only attains to probability in the case of rare and exceptional products of animal and vegetable life. Thus valerianic acid, which for a long time was extracted from the root of the *Valeriana officinalis*, could now probably be more cheaply prepared from its elements; but a still cheaper source of this acid has been in the meantime discovered, viz. the oxidation of amylic alcohol, a waste product formed in the manufacture of spirit of wine, and obtainable at such a moderate cost as to prevent, in an economical point of view, the successful production either of amylic alcohol or valerianic acid by any artificial and exclusively non-vital processes at present known. It is also highly probable, that if we could produce artificially such bodies as quinine and the rare alkaloids, or alizarine and similar powerful and valuable organic colouring matters, we should be able to compete with organic life in the formation of these bodies; nevertheless, the discovery of the processes of artificial formation would doubtless be preceded by a knowledge of methods, by which such rare bodies could be produced from more abundant, and consequently cheaper forms of vegetable or animal matter; and it is therefore exceedingly improbable, that any purely non-vital process will be successfully, and at the same time economically employed for the manufacture even of such rare and valuable vital products. Such being the economical bearings of the case with regard to the rare and exceptional educts of vitality, when we turn to consider the great staple products of the animal and vegetable kingdoms, the hope of rivalling natural processes becomes faint indeed. By no processes at present known could we produce sugar, glycerine, or alcohol from their elements, at one hundred times their present cost, as obtained through the agency of vitality. But, although our present prospects of rivalling vital processes in the economical production of staple organic compounds, such as those constituting the food of man, are so exceedingly slight, yet it would be rash to pronounce their ultimate realization impossible. It must be remembered, that this branch of chemistry is as yet in its merest infancy, and that it has hitherto attracted the attention of few minds; and further, that many analogous substitutions of artificial for natural processes have been achieved. Thus, under certain circumstances, we find it less economical to propel our ships

by the force of the wind, and our carriages by animal power, than to employ steam power for these purposes. We do not find it desirable to wait for the bleaching of our calicoes by the sun's rays; and even the grinding of corn is no longer entirely confided to wind and water power.

In such cases, where contemporaneous natural agencies have been superseded, we have almost invariably drawn upon that grand store of force collected by the plants of bygone ages, and conserved in our coal fields. It is the solar heat of a past epoch that furnishes the power which we now utilise in our steam-engines. One important element in cheap production is *time*, and it is precisely in regard to this element, that we economically supersede, in the above instances, the contemporary resources of nature. Now time is also an important element in the natural production of food; and although it is true, that the amount of labour required for the production of a given weight of food is not considerable, yet it is nevertheless true that this weight requires a whole year for its production. By the vital process of producing food we can only have one harvest in each year. But if we were able to form that food from its elements without vital agency, there would be nothing to prevent us from obtaining a harvest every week; and thus we might, in the production of food, supersede the present vital agencies of nature, as we have already done in other cases, by laying under contribution the accumulated forces of past ages, which would thus enable us to obtain in a small manufactory, and in a few days, effects which can be realized from present natural agencies, only when they are exerted upon vast areas of land, and through considerable periods of time.

[E. F.]

WEEKLY EVENING MEETING,

Friday, June 4.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President, in the Chair.

JOHN TYNDALL, ESQ. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On the Mer-de-Glace.

A PORTION of a series of observations made upon the Mer-de-Glace of Chamouni during the months of July and August last year formed the basis of this discourse.

The law first established by Prof. J. D. Forbes, that the central portions of a glacier moved faster than the sides, was amply illustrated by the deportment of lines of stakes placed across the Mer-de-Glace at several places, and across the tributaries of the glacier. The portions of the Mer-de-Glace derived from these tributaries were easily traceable throughout the glacier by means of the *moraines*. Thus, for example, that portion of the trunk stream derived from the Glacier du Géant, might be distinguished in a moment from the portion derived from the other tributaries, by the absence of the debris of the moraines upon the surface of the former. The commencement of the dirt formed a distinct junction between both portions. Attention has been drawn by Prof. Forbes to the fact, that the eastern side of the glacier in particular, is "excessively crevassed;" and he accounts for this crevassing by supposing that the Glacier du Géant moves most swiftly, and in its effort to drag its more sluggish companions along with it, tears them asunder, and thus produces the fissures and dislocation for which the eastern side of the glacier is remarkable. The speaker said that too much weight must not be attached to this explanation. It was one of those suggestions which are perpetually thrown out by men of science during the course of an investigation, and the fulfilment or non-fulfilment of which cannot materially affect the merits of the investigator. Indeed, the merits of Forbes must be judged on far broader grounds; and the more his labours are compared with those of other observers, the more prominently does his comparative intellectual magnitude come forward. The speaker would not content himself with saying that the book of Prof. Forbes was the best book which had been written upon the subject. The qualities of mind, and the physical culture invested in that excellent work, were such as to make it, in the estimation of the physical investigator at least, outweigh all other books upon the subject taken together. While thus acknowledging its merits, let a free and frank comparison of its statements with facts be instituted. To test whether the Glacier du Géant moved quicker than its fellows, five different lines were set out across the Mer-de-Glace, in the vicinity of the Montenvert, and in each of these it was found that the point of swiftest motion did not lie upon the Glacier du Géant at all; but was displaced so as to bring it comparatively close to the eastern side of the glacier. These measurements prove that the statement referred to is untenable; but the deviation of the point of swiftest motion from the centre of the glacier will doubtless be regarded by Prof. Forbes as of far greater importance to his theory. At the place where these measurements were made, the glacier turns its convex curvature to the eastern side of the valley, being concave towards the Montenvert. Let us take a bolder analogy than even that suggested in the explanation of Forbes, where he compares the Glacier du Géant to a strong and swiftly flowing river. Let us inquire how a river would behave in sweeping round a curve similar to that here existing. The point of swiftest motion

would undoubtedly lie on that side of the centre of the stream towards which it turns its convex curvature. Can this be the case with the ice? If so, then we ought to have a shifting of the point of maximum motion towards the eastern side of the valley, when the curvature of the glacier so changes as to turn its convexity to the western side. Such a change of flexure occurs opposite the passages called *Les Ponts*, and at this place the view just enunciated was tested. It was soon ascertained that the point of swiftest motion here lay at a different side of the axis from that observed lower down. But to confer strict numerical accuracy upon the result, stakes were fixed at certain distances from the western side of the glacier, and others *at equal distances* from the eastern side. The velocities of these stakes were compared with each other, two by two; a stake on the western side being always compared with a second one which stood at the same distance from the eastern side. The results of this measurement are given in the following table, the numbers denoting inches:—

1st pair.	2nd pair.	3rd pair.	4th pair.	5th pair.
West 15	West $17\frac{1}{4}$	West $22\frac{1}{4}$	West $23\frac{3}{4}$	West $23\frac{3}{4}$
East $12\frac{1}{2}$	East $15\frac{1}{4}$	East $15\frac{1}{2}$	East $18\frac{1}{4}$	East $19\frac{1}{2}$

It is here seen that in each case the western stake moved more swiftly than its eastern fellow stake; thus proving, beyond a doubt, that opposite the Ponts the western side of the Mer-de-Glace moves swiftest; a result precisely the reverse of that observed where the curvature of the valley was different.

But another test of the explanation is possible. Between the Ponts and the promontory of Trelaporte, the glacier passes a point of contrary flexure, its convex curvature opposite to Trelaporte being turned towards the base of the Aiguille du Moine, which stands on the eastern side of the valley. A series of stakes was placed across the glacier here; and the velocities of those placed at certain distances from the western side were compared, as before, with those of stakes placed at the same distances from the eastern side. The following table shows the result of these measurements; the numbers as before denote inches:—

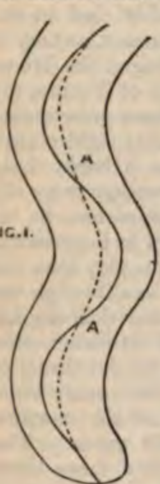
1st pair.	2nd pair.	3rd pair.
West . $12\frac{3}{4}$	West . 15	West . $17\frac{1}{4}$
East . $14\frac{3}{4}$	East . $17\frac{1}{2}$	East . 19

Here we find that in each case the *eastern* stake moved faster than its fellow. The point of maximum motion has therefore once more crossed the axis of the glacier, being now upon its eastern side.

Determining the points of maximum motion for a great number of transverse sections of the Mer-de-Glace, and uniting these points, we have the *locus* of the curve described by the

point referred to. Fig. 1 represents a sketch of the Mer-de-Glace. The dotted line is drawn along the centre of the glacier; the defined line which crosses the axis of the glacier at the points A A is then the locus of the point of swiftest motion. It is a curve more deeply sinuous than the valley itself, and crosses the central line of the valley at each point of contrary flexure. The speaker drew attention to the fact that the position of towns upon the banks of rivers is usually on the convex side of the stream, where the rush of the water renders silting-up impossible: the Thames was a case in point; and the same law which regulated its flow and determined the position of the adjacent towns, is at this moment operating with silent energy among the Alpine glaciers.

FIG. I.



Another peculiarity of glacier motion is now to be noticed.

Before any observations had been made upon the subject, it was surmised by Prof. Forbes that the portions of a glacier near its bed were retarded by friction against the latter. This view was afterwards confirmed by his own observations, and by those of M. Martins. Nevertheless the state of our knowledge upon the subject rendered further confirmation of the fact highly desirable. A rare opportunity for testing the question was furnished by an almost vertical precipice of ice, constituting the side of the Glacier du Géant, which was exposed near the Tacul. The precipice was about 140 feet in height. At the top and near the bottom stakes were fixed, and by hewing steps in the ice, the speaker succeeded in fixing a stake in the face of the precipice at a point about 40 feet above the base. After the lapse of a sufficient number of days, the progress of the three stakes was measured; reduced to the diurnal rate, the motion was as follows:—

Top stake	6·00 inches.
Middle stake . . .	4·59 ,,
Bottom stake . . .	2·56 ,,

We thus see that the top stake moved with more than twice the velocity of the bottom one; while the velocity of the middle stake lies between the two. But it also appears that the augmentation of velocity upwards is not proportional to the distance from the bottom, but increases in a quicker ratio. At a height of 100 feet from the bottom, the velocity would undoubtedly be practically the same as at the surface. Measurements made upon an adjacent ice cliff proved this. We thus see the perfect validity of the reason assigned by Forbes for the continued verticality of the walls of transverse crevasses. Indeed a comparison of the result

with his anticipations and reasonings will prove alike their sagacity and their truth.

The most commanding view of the Mer-de-Glace and its tributaries is obtained from a point above the remarkable cleft in the mountain range underneath the Aiguille de Charmoz, which is sure to attract the attention of an observer standing at the Montenvert. This point, which is marked G on the map of Forbes, the speaker succeeded in attaining. A Tübingen Professor once visited the glaciers of Switzerland, and seeing these apparently rigid masses enclosed in sinuous valleys, went home and wrote a book, flatly denying the possibility of their motion. An inspection from the point now referred to would have doubtless confirmed him in his opinion; and indeed nothing can be more calculated to impress the mind with the magnitude of the forces brought into play than the squeezing of the three tributaries of the Mer-de-Glace through the neck of the valley at Trélaporte. But let us state numerical results. Previous to its junction with its fellows, the Glacier du Géant measures 1134 yards across. Before it is influenced by the thrust of the Talèfre, the Glacier de Léchaud has a width of 825 yards; while the width of the Talèfre branch across the base of the cascade, before it joins the Léchaud, is approximately 638 yards. The sum of these widths is 2597 yards. At Trélaporte those three branches are forced through a gorge 893 yards wide, with a central velocity of 20 inches a day! The result is still more astonishing, if we confine our attention to one of the tributaries,—that of the Léchaud. Before its junction with the Talèfre, the glacier has a width of 37½ English chains. At Trélaporte this broad ice river is squeezed to a driblet of less than 4 chains in width, that is to say, to about one-tenth of its previous horizontal transverse dimension.

Whence is the force derived which drives the glacier through the gorge? The speaker believed that it must be a pressure from behind. Other facts also suggest that the Glacier du Géant is throughout its length in a state of forcible longitudinal compression. Now, taking a series of points along the axis of this glacier,—if these points, during the descent of the glacier, preserved their distances asunder perfectly constant, there could be no longitudinal compression. The mechanical meaning of this term, as applied to a substance capable of yielding like ice, must be that the hinder points are incessantly advancing upon the forward ones. The speaker was particularly anxious to test this view, which first occurred to him from *a priori* considerations. Three points, A B C, were therefore fixed upon the axis of the Glacier du Géant, A being the highest up the glacier. The distance between A and B was 545 yards; and that between B and C was 487 yards. The daily velocities of these three points, determined by the theodolite, were as follows:—

A	. .	20·55 inches.
B	. .	15·43 ,,
C	. .	12·75 ,,

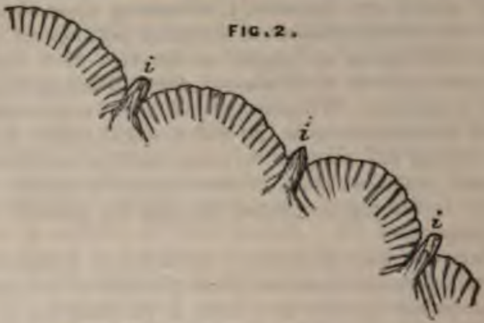
The result completely corroborates the foregoing anticipation. The hinder points are incessantly advancing upon those in front, and that to an extent sufficient to shorten a segment of this glacier, measuring 1000 yards in length, at the rate of 8 inches a day. Were this rate uniform at all seasons, the shortening would amount to 240 feet in a year. When we consider the compactness of this glacier, and the uniformity in the width of the valley which it fills, this result cannot fail to excite surprise; and the exhibition of force thus rendered manifest, must, in the speaker's opinion, be mainly instrumental in driving the glacier through the jaws of the granite vice at Trélaporte.

Attention was next drawn to a remarkable system of seams of white ice, which, when observed from a sufficient distance, appears to sweep across the Glacier du Géant, in the direction of the "dirt-bands." These seams are more resistant than the ordinary ice of the glacier, and sometimes protrude above the latter to a height of 3 or 4 feet. Their origin was for some time a difficulty, and it was at the base of the ice cascade which descends from the basin of the Talèfre, that the key to their solution first presented itself. It was well known that the ice of a glacier is not of homogeneous structure, but that the general more or less milky mass of the ice is traversed by blue veins of a more compact and transparent texture. In the upper portions of the Mer-de-Glace, these veins swept across the glacier in gentle curves, leaning forward,—to which leaning forward, Prof. Forbes gave the name of the "frontal dip." So far as the speaker was informed, a case of "backward dip" had never been described. Yet here, at the base of the ice cascade referred to, he had often noticed the veins exposed upon the walls of a longitudinal crevasse to lean backwards and forwards on both sides of a vertical line, like the joints of stones used to turn an arch. This fact was found to connect itself in the following way with the general state of the glacier. At the base of the ice-fall a succession of protuberances, with steep frontal slopes, followed each other, and were intersected by crevasses. Let the hand be placed flat upon the table, with the palm downwards; let the fingers be bent so as to render the space between the joints nearest the nails and the ends of the fingers nearly vertical. Let the second hand be now placed upon the back of the first, with its fingers bent as in the former case, and so placed that their ends rest upon the roots of the first fingers. The crumpling of the hands fairly represents the crumpling of the ice, and the spaces between the fingers represent the crevasses by which the protuberances are intersected. On the walls of these crevasses the change of dip of the veined structure before referred to was always observed, and *at the base of each protuberance a vein of white ice was found firmly wedged into the mass of the glacier.*

Fig. 2 represents a series of these crumples with the veins of white ice *i i i* at their bases. It was soon observed that the water which trickled down the protuberances, and gushed here and there

from glacier orifices collected at the bases of the crumples, and

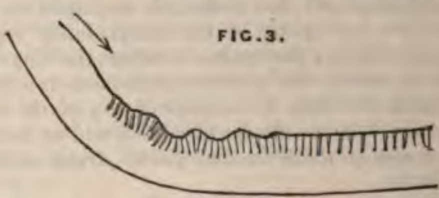
FIG. 2.



formed streams which cut for themselves deep channels in the ice. These streams seemed to form the exact matrices or moulds of the veins of white ice, and the latter were finally traced to the gorging up of the channels of glacial rivulets in winter by snow, and the subsequent compression of the mass to resistant white ice during the descent of the glacier. The same explanation applies to the system of bands upon the Glacier du Géant; and the speaker was enabled to trace the little arms of white ice which once were the tributaries of the streams, to see the vein of white ice dividing into branches, and uniting again so as to enclose glacial islands; he finally traced them to the region of their formation, and by sketches of existing streams, taken near the base of the *Seracs*, and of bands of white ice taken lower down, a resemblance so striking was exhibited as to leave no doubt of the connexion between both. On the walls of some deep crevasses which intersected the white ice seams at right angles he also found that the latter penetrated the glacier only to a limited depth; having the appearance of a kind of glacial "trap" intruded from above.

But how is the backward dip of the blue veins to be accounted for? The speaker believed in the following way. At the base of the cascade the glacier is forcibly compressed by the thrust of the mass behind it; besides this, it changes its inclination suddenly and considerably; it is bent upwards: and the consequence of this

FIG. 3.



bending is a system of wrinkles, such as those represented in Fig. 3. The interior of a bent umbrella handle, sometimes pre-

sents wrinkles which are the representatives, in little, of the protuberances upon the glacier. Or the coat sleeve is an equally instructive illustration: when the arm is bent at the elbow the sleeve wrinkles, and as the places where these wrinkles occur in the cloth are determined, to some extent, by the previous creasing, so also the places where the wrinkles are formed upon the glacier, are determined by the previous scarring of the ice during its descent down the cascade. The manner in which these crumples tend to *scale off* speaks strongly in favour of the explanation given. Fig. 4 is a type of numerous instances of scaling off observed by the speaker, and recorded in his note-book. By means of a hydraulic press he was able to produce a perfectly similar scaling off in small masses of ice. One consequence of this crumpling of the glacier would be the backward and forward inclination of the veins as actually observed. The falling backward and forward of the veins was also observed on the wrinkles of the Glacier du Géant. It was also proved, by measurements, that these wrinkles *shorten* as they descend.

FIG. 4.

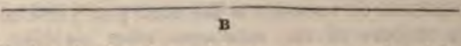
In virtue of what quality then can ice be bent and squeezed, and change its form in the manner indicated in the foregoing observations? The only theory worthy of serious consideration at the present day is that of Prof. Forbes, which attributes these effects to the viscosity of the ice. The speaker did not agree with this theory; as the term viscosity appeared to him to be wholly inapplicable as expressive of the physical constitution of glacier ice. He had already moulded ice into cups, bent it into rings, changed its form in a variety of ways by artificial pressure, and he had no doubt of his ability to mould a compact mass of Norway ice which stood upon the table into a statuette; but would viscosity be the proper term to apply to the process of bruising and regelation by which this result could be attained? He thought not. A mass of ice at 32° is very easily crushed, but it has as sharp and definite a fracture as a mass of glass. There is no sensible evidence of viscosity.

The very essence of viscosity is the ability of yielding to a force of *tension*, the texture of the substance, after yielding, being in a state of equilibrium, so that it has no strain to recover from; and the substances chosen by Prof. Forbes, as illustrative of the physical condition of a glacier, possess this power of being drawn out in a very eminent degree. But it has been urged, and justly urged, that we ought not to conclude that viscosity is absent because hand specimens do not show it, any more than we ought to conclude that ice is not blue because small fragments of the substance do not exhibit this colour. To test the question of viscosity then, we must appeal to the glacier itself. Let us do so. First, an analogy between the motion of a glacier through a sinuous valley,

and of a river in a sinuous channel has been already pointed out. But the analogy fails in one important particular: the river, and much more so a mass of flowing treacle, honey, tar, or melted caoutchouc, sweeps round its curves without rupture of continuity. The viscous mass *stretches*, but the icy mass *breaks*, and the "excessive crevassing" pointed out by Prof. Forbes himself, is the consequence. Secondly, the inclinations of the Mer-de-Glace and its three tributaries were taken, and the association of transverse crevasses with the changes of inclination was accurately noted. Every traveller knows the utter dislocation and confusion produced by the descent of the Mer-de-Glace from the Chapeau downwards. A similar state of things exists in the ice-cascade of the Talèfre. Descending from the Jardin, as the ice approaches the fall, great transverse chasms are formed, which at length follow each other so speedily as to reduce the ice masses between them to mere plates and wedges, along which the explorer has to creep cautiously. These plates and wedges are in some cases bent and crumpled by the lateral pressure, and on some masses vertical forces appeared to have acted turning large pyramids 90° round, so as to set their structure at right angles to its normal position. The ice afterwards descends the fall, the portions exposed to view being a fantastic assemblage of frozen boulders, pinnacles, and towers, some erect, some leaning, falling at intervals with a sound like thunder, and crushing the ice crags on which they fall to powder. The descent of the ice through this fall has been referred to as a proof of its viscosity; but the description just given does not, it was believed, harmonize with our ideas of a viscous substance.

But the proof of the non-viscosity of the substance must be sought at places where the change of inclination is very small. Nearly opposite l'Angle there is a change from 4 to 9 degrees, and the consequence is a system of transverse fissures which renders the glacier here perfectly impassable. Further up the glacier, transverse crevasses are produced by a change of inclination from 3 to 5 degrees. This change of inclination is accurately protracted in Fig. 5; the bend occurs at the point B; it is scarcely percepti-

Fig. 5.



ble, and still the glacier is unable to pass over it without breaking across. Thirdly, The crevasses are due to a state of strain from which the ice relieves itself by breaking: the rate at which they widen may be taken as a measure of the amount of relief demanded by the ice. Both the suddenness of their formation, and the slowness with which they widen are demonstrative of the non-viscosity of the ice. For were the substance capable of stretching even at the small rate at which they widen there would be no necessity for their formation.

Further, the marginal crevasses of a glacier are known to be a consequence of the swifter flow of its central portions, which throws the sides into a state of strain from which they relieve themselves by breaking. Now it is easy to calculate the amount of stretching demanded of the ice in order to accommodate itself to the speedier central flow. Take the case of a glacier, half a mile wide. A straight transverse element, or slice, of such a glacier, is bent in twenty-four hours to a curve. The ends of the slice move a little, but the centre moves more: let us suppose the versed side of the curve formed by the slice in twenty-four hours to be a foot, which is a fair average. Having the chord of this arc, and its versed side we can calculate its length. In the case of the Mer-de-Glace which is about half-a-mile wide, the amount of stretching demanded would be about the eightieth of an inch in twenty-four hours. Surely, if the glacier possessed a property which could with any propriety be called viscosity, it ought to be able to respond to this moderate demand; but it is not able to do so; instead of stretching as a viscous body, in obedience to this slow strain, it breaks as an eminently fragile one, and marginal crevasses are the consequence. It may be urged that it is not fair to distribute the strain over the entire length of the curve: but reduce the distance as we may, a residue must remain which is demonstrative of the non-viscosity of the ice.

To sum up then, two classes of facts present themselves to the glacier investigator, one class in harmony with the idea of viscosity, and another as distinctly opposed to it. Where *pressure* comes into play we have the former, where *tension* comes into play we have the latter. Both classes of facts are reconciled by the assumption, or rather the experimental verity, that the fragility of ice* and its power of regelation, render it possible for it to change its form without prejudice to its continuity; and no doubt was entertained that the motion of the parts of a glacier was aided by the partial liquefaction of the mass by pressure, as pointed out by Mr. James Thomson, and proved experimentally by Prof. Wm. Thomson, and the speaker himself.

A detailed account of the observations referred to in the foregoing pages is contained in a paper recently presented to the Royal Society. It now remains for the author to express his grateful sense of the able and unremitting assistance rendered him by his friend Mr. T. A. Hirst, during his six weeks' residence at the Montenvert.

[J. T.]

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* Wherever the compressed ice is surrounded by a resistant mass, the yielding is so gradual as to resemble *plasticity;* I should have no objection to the use of this term, but the term "*viscous*" has undoubtedly led to erroneous conceptions of the physical qualities of glacier ice.

GENERAL MONTHLY MEETING,

Monday, June 7.

THE LORD ASHBURTON, D.C.L. F.R.S. Vice-President, in the Chair.

Professor Thomas Minchin Goodeve, M.A.
James Johnston, Esq.
Mrs. Portlock, and
Miss Anne Swanwick.

were duly *elected* Members of the Royal Institution.

The following PRESENTS were announced, and the thanks of the Members returned for the same:—

FROM

Hon. East India Company—Bombay Observations for 1856. 4to. 1857.
Airy, G. B. F.R.S. (Astronomer-Royal)—Description of the Galvanic Chronographic Apparatus at the Royal Observatory, Greenwich. 4to. 1858.
Astronomical Society, Royal—Monthly Notices, May, 1858.
Author, The—The Social Evil. 8vo. 1858.
Bradbury, Henry, Esq. M.R.I. (the Author)—Printing: its Dawn, Day, and Destiny. 4to. 1858.
British Architects, Royal Institute of—Proceedings in May 1858. 4to.
Editors—The Medical Circular for May 1857. 8vo.
The Practical Mechanic's Journal for May 1858. 4to.
The Journal of Gas-Lighting for May 1858. 4to.
The Mechanics' Magazine for May 1858. 8vo.
The Athenæum for May 1858. 4to.
The Engineer for May 1858. fol.
The Artizan for May 1858.
Faraday, Professor.—Reale Accademia delle Scienze, Napoli:—Memorie, Vol. I. fasc 2. Vol. II. 1857. Rendiconto. 1856–7. 4to.
Geological Society—Proceedings, May, 1858.
Quarterly Journal, No. 54. 8vo. 1858.
Greenwich, Royal Observatory—Observations in 1856. 4to. 1858.
Linnean Society—Proceedings, No. 8. 8vo. 1858.
Logan, Sir W. E. (the Director)—Geological Survey of Canada. Report for 1853-6. 8vo. and 4to. 1857.
Lunacy Commissioners—Tenth and Eleventh Reports. 8vo. 1856–7.
Mendicity Society—Fortieth Annual Report. 8vo. 1858.
Robin, M. Ed.—Loi Nouvelle régissant les Différentes Propriétés Chimiques, &c. 8vo. Paris, 1853.
Royal Society of Literature—Transactions, Vol. VI. Part 1. 8vo. 1857.
Taylor, Alfred S. M.D. F.R.S. M.R.I. (the Author)—Medical Jurisprudence. 6th Ed. 16to. 1858.
Wilson, Thos. Esq. M.R.I.—Etudes sur les Fécules les plus usitées, par Dr. Saugerres. 8vo. 1858.
Yates, James, Esq. F.R.S. M.R.I. (the Author)—What is the best Unit of Length? 8vo. 1858.

WEEKLY EVENING MEETING,

Friday, June 11.

The Duke of Northumberland, K.G. F.R.S. President, in the Chair.

Professor Faraday, D.C.L. F.R.S.

On Wheatstone's Electric Telegraph in relation to Science (being an argument in favour of the full recognition of Science as a branch of Education).

The development of the applications of physical science in modern times has become so large and so essential to the well-being of man that it may justly be used, as illustrating the true character of pure science, as a department of knowledge, and the claims it may have for consideration by Governments, Universities, and all bodies to whom is confided the fostering care and direction of learning. As a branch of learning, men are beginning to recognize the right of science to its own particular place;—for though flowing in channels utterly different in their course and end to those of literature, it conduces not less, as a means of instruction, to the discipline of the mind; whilst it ministers, more or less, to the wants, comforts, and proper pleasure, both mental and bodily, of every individual of every class in life. Until of late years, the education for, and recognition of, it by the bodies which may be considered as governing the general course of all education, have been chiefly directed to it only as it could serve professional services,—namely, those which are remunerated by society; but now the fitness of university degrees in science is under consideration, and many are taking a high view of it, as distinguished from literature, and think that it may well be studied for its own sake, *i.e.* as a proper exercise of the human intelligence, able to bring into action and development all the powers of the mind. As a branch of learning, it has (without reference to its applications) become as extensive and varied as literature; and it has this privilege, that it must ever go on increasing. Thus it becomes a duty to foster, direct, and honour it, as literature is so guided and recognised; and the duty is the more imperative, as we find by the unguided progress of science and the experience it supplies, that of those men who devote themselves to studious education, there are as many whose minds are constitutionally disposed to the studies supplied by it, as there are of others more fitted by inclination and power to pursue literature.

The value of the public recognition of science as a leading branch of education, may be estimated in a very considerable degree by observation of the results of the education which it has obtained incidentally from those, who pursuing it, have educated themselves. Though men may be specially fitted by the nature of their minds for the attainment and advance of literature, science, or the fine arts, all these men, and all others, require first to be educated in that which is known in these respective mental paths; and when they go beyond this preliminary teaching, they require a self-education directed (at least in science) to the highest reasoning power of the mind. Any part of pure science may be selected to show how much this private self-teaching has done, and by that to aid the present movement in favour of the recognition generally of scientific education in an equal degree with that which is literary; but perhaps electricity, as being the portion which has been left most to its own development, and has produced as its results the most enduring marks on the face of the globe, may be referred to. In 1800, Volta discovered the voltaic pile; giving a source and form of electricity before unknown. It was not an accident, but resulted from his mental self-education: it was, at first, a feeble instrument, giving feeble results; but by the united mental exertions of other men, who educated themselves through the force of thought and experiment, it has been raised up to such a degree of power as to give us light, and heat, and magnetic and chemical action, in states more exalted than those supplied by any other means.

In 1819, Oersted discovered the magnetism of the electric current, and its relation to the magnetic needle; and as an immediate consequence, other men, as Arago and Davy, instructing themselves by the partial laws and action of the bodies concerned, magnetized iron by the current. The results were so feeble at first as to be scarcely visible; but, by the exertion of self-taught men since then, they have been exalted so highly as to give us magnets of a force unimaginable in former times.

In 1831, the induction of electrical currents one by another, and the evolution of electricity from magnets was observed,—at first in results so small and feeble, that it required one much instructed in the pursuit, to perceive and lay hold of them; but these feeble results, taken into the minds of men already partially educated and ever proceeding onwards in their self-education, have been so developed, as to supply sources of electricity independent of the voltaic battery or the electric machine, yet having the power of both, combined in a manner and degree which they, neither separate nor together, could ever have given it, and applicable to all the practical electrical purposes of life.

To consider all the departments of electricity fully, would be to lose the argument for its fitness in subserving education, in the vastness of its extent; and it will be better to confine the attention to one application, as the electric telegraph, and even to one small

part of that application, in the present case. Thoughts of an electric telegraph came over the minds of those who had been instructed in the nature of electricity as soon as the conduction of that power with extreme swiftness through metals was known, and grew as the knowledge of that branch of science increased. The thought, as realized at the present day, includes a wonderful amount of study and development. As the end in view presented itself more and more distinctly, points at first apparently of no consequence to the knowledge of the science generally, rose into an importance, which obtained for them the most careful culture and examination, and the almost exclusive exercise of minds whose powers of judgment and reasoning had been raised first by general education, and who, in addition, had acquired the special kind of education which the science in its previous state could give. Numerous and important as the points are, which have been already recognised, others are continually coming into sight as the great development proceeds, and with a rapidity such as to make us believe that much as there is known to us, the unknown far exceeds it; and that extensive as is the teaching of method, facts, and law, which can be established at present, an education looking for far greater results should be favoured and directed.

The results already obtained are so large, as even in money value to be of very great importance;—as regards their higher influence upon the human mind, especially when that is considered in respect of cultivation, I trust they are, and ever will be, far greater. No intention exists here of comparing one telegraph with another, or of assigning their respective dates, merits, or special uses. Those of Mr. Wheatstone are selected for the visible illustration of a brief argument in favour of a large public recognition of scientific education, because he is a man both of science and practice, and was one of the very earliest in the field, and because certain large steps in the course of his telegraphic life will tell upon the general argument. Without referring to what he had done previously, it may be observed that in 1840 he took out patents for electric telegraphs, which included, amongst other things, the use of the electricity from magnets at the communicator,—the dial face,—the step-by-step motion,—and the electro-magnet at the indicator. At the present time, 1858, he has taken out patents for instruments containing all these points; but these instruments are so altered and varied in character above and beyond the former, that an untaught person could not recognise them. The changes may be considered as the result of education upon the one mind which has been concerned with them, and are to me strong illustrations of the effects which general scientific education may be expected to produce.*

* The former and the present apparatus were set to work in illustration of the points as they were noticed.

In the first instruments powerful magnets were used, and keepers with heavy coils associated with them. When magnetic electricity was first discovered, the signs were feeble, and the mind of the student was led to increase the results by increasing the force and size of the instruments. When the object was to obtain a current sufficient to give signals through long circuits, large apparatus were employed, but these involved the inconveniences of inertia and momentum; the keeper was not set in motion at once, nor instantly stopped; and, if connected directly with the reading indexes, these circumstances caused an occasional uncertainty of action. Prepared by its previous education, the mind could perceive the disadvantages of these influences, and could proceed to their removal; and now a small magnet is used to send sufficient currents through 12, 20, 50, a hundred, or several hundred miles; a keeper and helix is associated with it, which the hand can easily put in motion; and the currents are not sent out of the indicating instrument to tell their story, until a key is depressed, and thus irregularity contingent upon first action is removed. A small magnet, ever ready for action and never wasting, can replace the voltaic battery; if powerful agencies be required, the electro-magnet can be employed without any change in principle or telegraphic practice; and as magneto-electric currents have special advantages over voltaic currents, these are in every case retained. These advantages I consider as the results of scientific education, much of it not tutorial, but of self: but there is a special privilege about the science-branch of education, namely, that what is personal in the first instance immediately becomes an addition to the stock of scientific learning, and passes into the hands of the tutor, to be used by him in the education of others, and enable them in turn to educate themselves. How well may the young man entering upon his studies in electricity be taught by what is past to watch for the smallest signs of action, new or old; to nurse them up by any means until they have gained strength; then to study their laws, to eliminate the essential conditions from the non-essential, and at last, to refine again, until the encumbering matter is as much as possible dismissed, and the power left in its highly developed and most exalted state.

The alternations or successions of currents produced by the movement of the keeper at the communicator, pass along the wire to the indicator at a distance; there each one for itself confers a magnetic condition on a piece of soft iron, and renders it attractive or repulsive of small permanent magnets; and these acting in turn on a propelment, cause the index to pass at will from one letter to another on the dial face. The first electro-magnets, *i.e.* those made by the circulation of an electric current round a piece of soft iron, were weak; they were quickly strengthened, and it was only when they were strong, that their laws and actions could be successfully investigated. But now they were required small, yet potential.

Then came the teaching of Ohm's law; and it was only by patient study under such teaching that Wheatstone was able so to refine the little electro-magnets at the indicator as that they should be small enough to consist with the fine work there employed, able to do their appointed work when excited in contrary directions by the brief currents flowing from the original common magnet, and unobjectionable in respect of any resistance they might offer to the transit of these tell-tale currents.

These small transitory electro-magnets attract and repel certain permanent magnetic needles, and the to-and-fro motion of the latter is communicated by a propelment to the index, being there converted into a step-by-step motion. Here everything is of the finest workmanship; the propelment itself requires to be watched by a lens, if its action is to be observed; the parts never leave hold of each other; the vibratory or rotatory rachet wheel and the fixed pallets are always touching, and thus allow of no detachment or loose shake; the holes of the axes are jewelled; the moving parts are most carefully balanced, a consequence of which is that agitation of the whole does not disturb the parts, and the telegraph works just as well when it is twisted about in the hands, or placed on board a ship or in a railway carriage, as when fixed immovably. Where it is possible, as in the vibratory needle, the moving parts are brought near to the centre of motion, that the inertia of the portion to be moved, or the momentum of that to be stopped, should be as small as possible, and thus great quickness of indication obtained. All this delicacy of arrangement and workmanship is introduced advisedly; for the inventor, whom I may call the student here, considers that refined and perfect workmanship is more exact in its action, more unchangeable by time and use, and more enduring in its existence, than that which being heavier must be coarser in its workmanship, less regular in its action, and less fitted for the application of force by fine electric currents.

Now there was no chance in the course of these developments;—if there were experiments, they were directed by the previously acquired knowledge;—every part of the investigations was made and guided by the instructed mind. The results being such, (and like illustrations might be drawn from other men's telegraphs or from other departments of electrical science,) then, if the term education may be understood in so large a sense as to include all that belongs to the improvement of the mind either by the acquisition of the knowledge of others or by increase of it through its own exertions, we learn by them what is the kind of education science offers to man. It teaches us to be *neglectful* of nothing;—not to despise the small beginnings, for they precede of necessity all great things in the knowledge of science, either pure or applied. It teaches a continual comparison of the *small and great*, and that under differences almost approaching the infinite: for the small as often contains the great in principle as the great does the small; and thus the

mind becomes comprehensive. It teaches to deduce principles carefully, to hold them firmly, or to suspend the judgment:—to discover and obey *law*, and by it to be bold in applying to the greatest what we know of the smallest. It teaches us first by tutors and books to learn that which is already known to others, and then by the light and methods which belong to science to learn for ourselves and for others;—so making a fruitful return to man in the future for that which we have obtained from the men of the past. Bacon, in his instruction, tells us that the scientific student ought not to be as the ant who gathers merely, nor as the spider who spins from her own bowels, but rather as the bee who both gathers and produces.

All this is true of the teaching afforded by any part of physical science. Electricity is often called wonderful—beautiful;—but it is so only in common with the other forces of nature. The beauty of electricity, or of any other force, is not that the power is mysterious and unexpected, touching every sense at unawares in turn, but that it is under *law*, and that the taught intellect can even now govern it largely. The human mind is placed above, not beneath it; and it is in such a point of view that the mental education afforded by science is rendered supereminent in dignity, in practical application, and utility; for, by enabling the mind to apply the natural power through law, it conveys the gifts of God to man.

[M. F.]

GENERAL MONTHLY MEETING,

Monday, July 5.

THE LORD ASHBURTON, D.C.L. F.R.S. Vice-President, in the Chair.

James Don, M.D.

was duly *elected* a Member of the Royal Institution.

Robert Tait, Esq.

was *admitted* a Member of the Royal Institution.

The Secretary announced, That the Managers had appointed PROFESSOR RICHARD OWEN, D.C.L. F.R.S. to be Fullerian Professor of Physiology, on June 14th last.

The following Presents were announced, and the thanks of the Members returned for the same :—

From

Anonymous—Dr. S. G. Howe and others on the Causes of Idiocy. 8vo. 1858.
Astronomical Society, Royal—Monthly Notices, June 1858. 8vo.
Bath and West of England Society—Journal, Vol. VI. 8vo. 1858.
Bell, Jacob, Esq. M.R.I.—Pharmaceutical Journal for May and June 1858. 8vo.
Boosey, Messrs. (the Publishers)—The Musical World for May and June 1858. 4to.
British Architects, Royal Institute of—Proceedings in June 1858. 4to.
Editors—The Medical Circular for June 1858. 8vo.
The Practical Mechanic's Journal for June 1858. 4to.
The Journal of Gas-Lighting for June 1858. 4to.
The Mechanics' Magazine for June 1858. 8vo.
The Athenæum for June 1858. 4to.
The Engineer for June 1858. fol.
The Artizan for June 1858.
De Brock, M. Ministre des Finances de Russie—Compte Rendu Annuel de l'Observatoire Physique Central de Russie, 1856. 4to. 1857.
Faraday, Professor D.C.L. F.R.S. &c.—Königliche Preussischen Akademie, Berichte, April, Mai, 1858.
Franklin Institute of Pennsylvania—Journal, Vol. XXXIV Nos. 4, 5. 8vo. 1858.
Geographical Society, Royal—Proceedings, Vol. II. No. 3. 8vo. 1858.
Geological Society—Proceedings, June 1858.
Graham, George, Esq. (Registrar-General)—Reports of the Registrar-General for May and June 1858. 8vo.
Lankester, Dr. E. M.R.I.—Küchenmeister's Parasites of the Human Body. Vol. II. 8vo. 1858.
Madden, Sir Frederick (the Author)—Examination of Mr. Singer's Remarks on the Glossary of Havelok the Dane. 4to. 1829.
Macilwain, George, Esq. M.R.I. (the Author)—Clinical Memoir on Strangulated Hernia, &c. 8vo. 1858.
Newton, Messrs.—London Journal (New Series), June 1858. 8vo.
Novello, Mr. (the Publisher)—Musical Times, for May and June 1858. 4to.
Petermann, A. Esq. (the Author)—Mittheilungen auf dem Gesammtgebiete der Geographie, 1858. Heft 3, 4. 4to. Gotha, 1858.
Photographic Society—Journal, Nos. 66, 67. 8vo. 1858.
Pittman, Josiah, Esq. (the Author)—The People in Church, &c. 8vo. 1858.
Royal Society of London—Proceedings, No. 31. 8vo. 1858.
Society of Arts—Journal for May and June 1858. 8vo.
Statistical Society—Journal, Vol. XXI. Part 2. 8vo. 1858.
Surrey Archæological Society—Collections. Vol. I. 8vo. 1857-58.
Trench, Sir Frederick (the Author)—Letter on the Thames Embankment. 4to. 1841.
Vereins zur Beförderung des Gewerbfleisses in Preussen—März und April 1858. 4to.
Walker, C. V. Esq.—The Globotype Telegraph, invented by David M'Callum. 8vo. 1858.

INDEX TO VOL. II.

END OF VOL. II.

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